

ESSAYS,

POLITICAL, ECONOMICAL,

AND

PHILOSOPHICAL.

VOL. II.

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ESSAYS,
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AND
PHILOSOPHICAL.

By BENJAMIN COUNT OF RUMFORD,
KNIGHT OF THE ORDERS OF THE WHITE EAGLE, AND ST. STANISLAUS;
*Chamberlain, Privy Counsellor of State, and Lieutenant-General in the Service
of his Most Serene Highness the ELECTOR PALATINE, Reigning DUKE
of BAVARIA; Colonel of his Regiment of Artillery, and Commander in
Chief of the General Staff of his Army; F. R. S. Acad. R.
Hiber. Berol. Elec. Boicæ. Palat. et Amer. Soc.*

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IN THE STRAND.

1798.

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ESSAY VI.
OF
THE MANAGEMENT OF FIRE,
AND
THE ECONOMY OF FUEL.

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ESSAY VI.

CHAP. I.

The Subject of this Essay curious and interesting in a very high degree.—All the Comforts, Conveniences, and Luxuries of Life, are procured by the Assistance of FIRE and of HEAT.—The Waste of Fuel very great.—Importance of the Economy of Fuel to Individuals, and to the Public.—Means used for estimating the Amount of the Waste of Fuel.—An Account of the first Kitchen of the House of Industry at Munich, and of the Expence of Fuel in that Kitchen, compared with the Quantity consumed in the Kitchens of private Families.—An Account of several other Kitchens constructed on various Principles at Munich, under the Direction of the Author.—Introduction to a more scientific Investigation of the Subject under consideration.

NO subject of philosophical inquiry, within the limits of human investigation, is more calculated to excite admiration, and to awaken curiosity, than FIRE; and there is certainly none more extensively useful to mankind. It is owing, no doubt, to our being acquainted with it from our infancy, that

that we are not more struck with its appearance, and more sensible of the benefits we derive from it. Almost every comfort and convenience which man by his ingenuity procures for himself, is obtained by its assistance; and he is not more distinguished from the brute creation by the use of speech, than by his power over that wonderful agent.

Having long been accustomed to consider the Management of Heat as a matter of the highest importance to mankind, a habit of attending carefully to every circumstance relative to this interesting subject that occasionally came under my observation, soon led me to discover how much this science has been neglected, and how much room there is for very essential improvements in almost all those various operations in which heat is employed for the purposes of human life.

The great waste of Fuel in all countries must be apparent to the most cursory observer; and the uses to which Fire is employed are so very extensive, and the expence for Fuel makes so considerable an article in the list of necessaries, that the importance of the subject cannot be denied.

And with regard to the Economy of Fuel, it has this in particular to recommend it, that whatever is saved by an individual, is at the same time a positive saving to the whole community; for the less demand there is for any article in the market, the lower will be its price; and as all the subjects of useful industry—all the arts and manufactures, without exception, depend, directly or indirectly, on operations in which Fire is necessary, it is of much

much importance to a manufacturing and commercial country to keep the price of Fuel as low as possible:—And even in countries where there are no manufactures, and where the inhabitants subsist entirely by agriculture, if wood be used as Fuel—as the proportion of woodland to arable must depend in a great measure on the consumption of fire-wood, any saving of Fuel will be attended with a proportional diminution of the forests reserved for fire-wood,—consequently, with an increase of the lands under cultivation,—with an increase of inhabitants,—and of national wealth, strength, and prosperity.

But what renders this subject peculiarly interesting, is the great relief to the poor in all countries, and particularly in all cold climates, and in all great cities in every climate, that would result from any considerable diminution of the price of Fuel, or from any simple contrivance by which a smaller quantity of this necessary article than they now are obliged to employ to make themselves comfortable, might be made to perform the same services. Those who have never been exposed to the inclemencies of the seasons,—who have never been eye-witnesses to the sufferings of the poor in their miserable habitations, pinched with cold and starving with hunger,—can form no idea of the importance to them of the subject which I propose to treat in this Essay.

To all those who take pleasure in doing good to mankind by promoting useful knowledge, and facilitating the means of procuring the comforts and

conveniencies of life, these investigations cannot but be very interesting.

Though it is generally acknowledged that there is a great waste of Fuel in all countries, arising from ignorance and carelessness in the management of Fire, yet few—very few, I believe—are aware of the real amount of this waste.

From the result of all my inquiries upon this subject, I have been led to conclude, that not less than *seven-eighths* of the heat generated, or which, *with proper management, might be generated*, from the Fuel actually consumed, is carried up into the atmosphere with the smoke, and totally lost. And this opinion has not been formed hastily; on the contrary, it is the result of much attentive observation, and of many experiments. But, in a matter of so much importance, I feel it to be my duty not merely to give the Public my *opinions*, but to lay before them the grounds upon which those opinions have been founded; in order that every one may judge for himself of the certainty, or probability, of my deductions.

It would not be difficult, merely from a consideration of the nature of heat,—of the manner in which it is generated in the combustion of Fuel, and the manner in which it exists when generated,—to shew that, as the process of boiling is commonly performed, there must of necessity be a very great loss of heat; for when the vessel, in which the fluid to be boiled is contained, is placed over an open or naked fire, not only by far the greater part of the radiant heat is totally lost, but also of that which
exists

exists in the flame, smoke, and hot vapour, a very small proportion only enters the vessel; the rest going off with great rapidity, by the chimney, into the higher regions of the atmosphere. But, without insisting upon these reasonings, (though they are certainly incontrovertible,) I shall endeavour to establish the facts in question upon still more solid ground—that of actual experiment.

In the prosecution of the experiments necessary in this investigation, I proceeded in the following manner:—As the quantity of heat which any given quantity of any given kind of Fuel is capable of generating, is not known, there is no fixed standard with which the result of an experiment can be compared, in order to ascertain exactly the proportion of the heat saved, or usefully employed, to that lost: Instead therefore of being able to determine this point *directly*, I was obliged to have recourse to *approximations*. Instead of determining the quantity of heat lost in any given operation, I endeavoured to find out with how much less Fuel the same operation might be performed, by a more advantageous arrangement of the Fire, and disposition of the machinery: And several extensive public establishments, which have been erected in Bavaria within these last six or seven years, under my direction, by order of His Most Serene Highness the ELECTOR PALATINE; particularly an establishment for the Poor of Munich (of which an account has been given to the Public in my first Essay); and the Establishment of a Public Academy for the education of 180 young men, destined for the service

of the state in the different civil and military departments;—the economical arrangements of these establishments afforded me a most favourable opportunity of putting into practice all my ideas relative to the Management of Fire; and of ascertaining, by numerous experiments made upon a large scale, and often varied and repeated, the real importance of the improvements I have introduced.

That many experiments have been actually made in these two Establishments, during the seven years they have existed, will not be doubted by those who are informed, that the Kitchen, or rather the Fire-place of the kitchen of the House of Industry, has been pulled down and built entirely anew no less than *three times*, and that of the Military Academy *twice*, during that period; and that the forms of the boilers, and the internal construction of the fire-places, have been changed still oftener.

The importance of the improvements in the management of heat employed in culinary operations, which have resulted from these investigations, will appear by comparing the quantity of Fuel now actually used in those kitchens, to that consumed in performing the same operations in kitchens on the common construction: And this will at the same time shew, in a clear and satisfactory manner, what I proposed to prove,—namely, that in all the common operations in which Fire is employed, there is a very great waste of Fuel.

The waste of Fuel in boiling water or any other liquid over an open fire, in the manner in which

that process is commonly performed, and the great saving of Fuel which will result from a more advantageous disposition and management of the Fire, will be evident from the results of the following Experiments, all of which were made by myself, and with the utmost care.

Experiment, No. 1.

A copper boiler belonging to the kitchen of the Military Academy in Munich, 22 Rhinland inches in diameter above, 19 $\frac{1}{2}$ inches in diameter below, and 24 inches in depth, and which weighed 50 lbs. weight of Bavaria, (= 61.92 lbs. avoirdupois,) being fixed in its fire-place, was filled with 95 Bavarian measures (= 28 English wine-gallons) of water, which weighed 187 Bavarian pounds (= 232.58 lbs. avoirdupois); and this water being at the temperature of 58° F. a fire was lighted under the boiler with dry beech-wood, and the water was made to boil, and was continued boiling two hours. The time employed and wood consumed in this Experiment, were as follows:

	Time employed.		Wood consumed.
	Hours.	Min.	lbs.
To make the water boil, - -	1	1	11
To keep the water boiling, - -	2	0	2 $\frac{1}{2}$
Total, -	3	1	13 $\frac{1}{2}$

Experi-

Experiment, No. 2.

The same boiler, containing the same quantity of water at the same temperature, being now removed to the kitchen of a private gentleman in the neighbourhood, (Baron de Schwachheim, a brother of the Commandant of the Academy,) and placed upon a tripod, a quantity of the same kind of wood used in the former Experiment being provided, a fire was lighted under it by the gentleman's cook, (directions having been given to be as sparing as possible of Fuel,) and it was made to boil, and continued boiling two hours.

The result of the Experiment was as follows :

	Time employed.		Wood consumed.
	Hours.	Min.	lbs.
To make the water boil,	1	31	45
To keep it boiling,	2	0	17½
Total,	3	31	62½

As in these two Experiments the same boiler was employed ;—as the quantity of water was the same, —as also its temperature at the beginning of the Experiments,—and as it was made to continue boiling during the same length of time, it is evident that the quantities of wood consumed show the relative advantages of the different methods employed in the management of the Fire. The difference of these quantities of Fuel is very great (the one being only 13½ lbs. and the other amounting to no

less than $62\frac{1}{2}$ lbs.). And this shows how very considerable the waste of Fuel really is, in the manner in which it is commonly employed for culinary purposes, and how important the savings are which may be made by introducing a more advantageous arrangement for the management of Fire. But great as these savings may appear to be, as shown by the results of the foregoing Experiments, yet they are in fact still more considerable, as will be abundantly proved in the sequel. In the Experiment, No. 2. in which the boiler was put over an open Fire, great care was taken to place the Fuel in the most advantageous manner; but, in general, little attention is paid to that circumstance, and the waste of Fuel is greatly increased by such negligence: But in closed fire-places, upon a good construction, as the *proper place* for the Fuel cannot be mistaken, and as it is fixed, and bounded on all sides by a wall, the ignorance or inattention of those who take care of the Fire can never be productive of any great waste of Fuel; and this is an advantage of no small importance attending these fire-places.

Experiment, No. 3.

A large copper sauce-pan or *casserole*, $11\frac{1}{4}$ inches in diameter above,— $10\frac{3}{4}$ in diameter below, and $3\frac{3}{4}$ inches deep, containing 4 measures of water, weighing $7\frac{1}{2}$ lbs. and at the temperature of 58° F. being placed in its closed fire-place, and a fire being made under it with small pieces of dry beech-wood cut in lengths of about 4 inches, the water was made to boil, and was continued boiling two hours.

The result of the Experiment was as follows:

	Time employed.		Wood consumed.
	Hours.	Min.	lbs.
To make the water boil, - -	0	12	1
To keep it boiling, - - -	2	0	$0\frac{3}{4}$
Total,	2	12	$1\frac{3}{4}$

Experiment, No. 4.

The same sauce-pan, containing the same quantity of water, and at the same temperature as in the last Experiment, was now taken from its proper fire-place, and placed upon a tripod; and a fire being made under it with dry beech-wood, the result of the Experiment was as follows:

	Time employed.		Wood consumed.
	Hours.	Min.	lbs.
To make the water boil, - -	0	28	6
To keep it boiling, - - -	2	0	$5\frac{1}{2}$
Total,	2	28	$11\frac{1}{2}$

The

The difference in the results of these two Experiments is nearly the same as that in the results of those before mentioned, and they all tend to show, that in cooking, or boiling over an open fire, nearly *five times* as much Fuel is required, as when the heat is confined in a closed fire-place, and its operation properly directed.

But I must again repeat, what I have already observed with respect to the two former Experiments, as the Experiments, No. 2. and No. 4. were both made with the utmost care, the results of them, compared with those which were made with the same boilers placed in closed fire-places, can give no adequate idea of the real loss of heat, and waste of Fuel, which take place in the common operations of cookery.

From several estimates which I have made with great care relative to this subject, founded upon the quantity of Fuel actually consumed in the kitchens of several private families, compared with the quantities of different kinds of food prepared for the table, it appears that at least *nine-tenths* of the wood actually consumed in common kitchens, where cooking is carried on over an open fire, might be saved, by introducing the various improvements I have brought into use in the kitchens which have been constructed under my directions.

But it is not alone in kitchens, in which cooking is carried on over open fires, that useful alterations may be made; kitchens with closed fire-places, and indeed all the kitchens which have yet been contrived,

trived, (as far as my knowledge extends,) are susceptible of great improvement.

The various improvements that may be made in mechanical arrangements for the Economy of Fuel, will appear in a striking manner from a detail of the different alterations which have from time to time been made in the kitchen of the House of Industry at Munich, and in that of the Military Academy, and of the effects produced by those progressive improvements.

The House of Industry being an establishment of public charity, and the number of those fed from the kitchen amounting from 1000 to 1500 persons daily, the Economy of Fuel, in a kitchen upon so large a scale, became an object of serious consideration; and I attended to this matter with peculiar pleasure, as it so completely coincided with my favourite philosophical pursuits.

The investigation of heat, and of the laws of its operations, had long occupied my attention, and I had been so fortunate, in the course of my Experiments upon that subject, as to make some discoveries which were thought worthy of being inserted in the Philosophical Transactions of the Royal Society of London; and for my last paper upon that subject, published in the Transactions for the year 1792, I had the honour to receive the annual Medal of the Society. I hope my mentioning this circumstance will not be attributed to ostentation. My motive in doing it, is merely to show, that when I undertook to make the arrangements of which I
am

am about to give an account, the subject was by no means new to me; but, on the contrary, that I was prepared, and in some measure qualified, for such investigation.

I conceive it to be the duty of those who propose useful improvements for the benefit of mankind, not only to *merit*, but also to do every thing in their power to *obtain*, the confidence of those to whom their proposals are submitted; and there appears to me to be a much greater degree of pride and arrogance displayed by an author *in taking it for granted* that the world is already sufficiently acquainted with his merit and his qualifications to treat the subject he undertakes to investigate, than in modestly pointing out the grounds upon which the confidence of the Public in his knowledge of his subject, and in his integrity, may be founded.

But to return from this digression. In the first arrangement of the kitchen in the House of Industry at Munich, which was finished in the beginning of the year 1790, eight large copper boilers, each capable of containing about 38 English wine gallons, were placed in such a manner in two rows, in a solid mass of brick-work, 3 feet high, 9 feet wide, and 18 feet long, built in the middle of the kitchen, that, from a single fire-place, situated at one end of this brick-work, by means of canals (furnished with valves or dampers) going from it, through the solid mass of the brick-work to all the different boilers, these boilers were all heated, and made to boil with one single fire; and though none of
them

them were in actual contact with the fire-place, and some of them were distant from it near 15 feet, yet they were all heated with great facility, and in a short space of time, by the heat which, upon opening the valves, (which were of iron,) was made to pass through the canals.

Each boiler having its separate canal, and its separate valves, any single boiler, or any number of them, might be heated at pleasure, without heating the rest; and by opening the valves of any boiler more or less, more or less heat, as the occasion required, might be made to pass under the boiler;—and when no more heat was wanting for any of the boilers, or when the fire was too strong, by opening a particular valve, a communication with a waste-canal was formed, by which all the heat, or any part of it at pleasure, might be made to pass off directly into the chimney, without going near any of the boilers.

The Fire was regulated by a register in the door of the ash-pit, by which the air was admitted into the fire-place; and, when no more heat was wanted, the Fire was put out by closing this register entirely, and by closing at the same time all the valves or dampers in the canals leading from the fire-place.

The fire-place was of an oval form, 3 feet long, 2 feet 3 inches wide, and about 18 inches high, vaulted above with a double vault, 4 inches of air being left between the two vaults; and the Fuel was introduced into the fire-place by a passage
closed

closed by a *double* iron door, which door was kept constantly shut;—and the Fuel was burnt upon an iron grate; the air which supplied the Fire coming up from below the grate through the ash-pit.

The loss of heat in its passage from the fire-place to the boilers, was prevented by making the canals of communication *double*, one within the other; the internal canal by which the heat passed, and which was 5 inches wide internally, and 6 inches high, being itself placed, and, as it were, *insulated*, in a canal still larger, in such a manner that the canal by which the heat passed, (which was constructed of very thin bricks, or rather tiles,) was *surrounded on every side* with a wall, 2 inches thick, of *confined air*. The surrounding canal being formed in the solid body of the mass of brick-work, this arrangement of the double canals was entirely concealed. The double canals and the double vault over the fire-place were intended to serve the same purpose, namely, *to confine more effectually the heat*, and prevent its escape into the mass of brick-work, and its consequent loss.

Having found, in the course of my Experiments, that confined air is the best barrier * that can be opposed to heat, to confine it I endeavoured to avail myself of that discovery in these economical arrangements, and my attempts were not unsuccessful.

Not only the fire-place itself, and the canals of communication between the fire-place and the boil-

* See Philosophical Transactions, 1793, Part I.

ers, were surrounded by confined air, but it was also made use of for confining the heat in the boilers, and preventing its escaping into the atmosphere. This was done by making the covers of the boilers *double*. These covers, (See the figures 1 and 2, Plate I.) which were made of tin, or rather of thin iron-plates tinned, were in the form of a hollow cone; the height of the cone was equal to about one-third of its diameter; and the air which it contained was entirely shut up, the bottom of the cone being closed by a circular plate or thin sheet of tinned iron. The bottom of the cone was accurately fitted to the top of the boiler, which it completely closed by means of a rim about 2 inches wide, which entered the boiler; which rim was folded to the flat sheet of tinned iron which formed the bottom of the cover. The steam, generated by the boiling liquid, was carried off by a tube about half an inch in diameter, which passed through the hollow conical cover, and which was attached to the cover, both above and below, with folder, in such a manner that the air with which the hollow cone was filled, remained completely confined, and cut off from all communications with the external air of the atmosphere, as well as with the steam generated in the boiler.

In some of the covers I filled the hollow of the cone with fur; but I did not find that these were sensibly better for confining the heat than those in which the cone was filled simply with air.

To convince the numerous strangers, who from curiosity visited this kitchen, of the great advantage of making use of double covers to confine the heat in the boilers, instead of using single covers for that purpose, a single cover was provided, which, as it was externally of the same form as the others, when it was placed upon a boiler, could not be distinguished from them; but as its bottom was wanting, and consequently there was no confined air interposed between the hot steam in the boiler and the external surface of the cover, on being placed upon a kettle actually boiling, this cover instantaneously became so exceedingly hot as actually to burn those who ventured to touch it;—while a *double cover*, formed of the same materials, and placed in the same situation, was so moderately warm that the naked hand might be held upon it for any length of time without the least inconvenience.

As it was easy to conceive that what was so exceedingly hot as to burn the hand, in an instant, upon touching it, could not fail to communicate a great deal of heat to the cold atmosphere, which continually lay upon it, this Experiment showed, in a striking and *convincing* manner, the utility of my double covers; and I have since had the satisfaction to see them gradually finding their way into common use.

It is perhaps quite unnecessary that I should inform my readers, that one principal motive which induced me to take so much pains in the arrangement of this kitchen, was a desire to introduce use-

ful improvements relative to the Management of Heat and the Economy of Fuel, into common practice. An establishment so interesting in all respects,—so important in its consequences,—and so perfectly new in Bavaria, as a public House of Industry upon a liberal and extensive plan,—where almost every trade and manufacture is carried on under the same roof,—where the poor and indigent of both sexes, and of all ages, find a comfortable asylum, and employment suited to their strength and to their talents; and where industry is excited, *not by punishments, but by the most liberal rewards,* and by the kindest usage: Such an establishment, I thought, could not fail to excite the curiosity of the Public, and to draw together a great concourse of visitors; and as this appeared to me a favorable opportunity to draw the public attention to useful improvements, all my measures were taken accordingly; and not only the kitchen, but also the bake-house,—the stoves for heating the rooms,—the lamps,—the various utensils and machines made use of in the different manufactories,—all the different economical arrangements and contrivances for facilitating the operations of useful industry, were so many models expressly made for imitation.

But in the arrangements relative to the Economy of Fuel, besides a view to immediate public utility, another motive, not much less powerful, contributed to induce me to pay all possible attention to the subject; namely, a desire to acquire a more thorough knowledge relative to the nature of Heat,
and

and of the laws of its operations; and with this view several parts were added to the machinery, which I suspected at the time to be too complicated to be really useful in common practice.

The steam, for instance, which arose from the boiling liquids, instead of being suffered to escape into the atmosphere, was carried up by tubes into a room immediately over the kitchen, where it was made to pass through a spiral worm, placed in a large cask full of cold water, and condensed, giving out its heat to the water in the cask; which water thus warmed, without any new expence of Fuel, was made use of next day, instead of cold water, for filling the boilers. That this water, so warmed, might not be cooled during the night, the cask that contained it was put into another cask still larger; and the space between the two casks was filled with wool. The cooling of the steam, in its passage from the boiler to the cask where it was condensed, was prevented by warm coverings of sheep-skins with the wool on them, by which the tubes of communication, which were of tin, were defended from the cold air of the atmosphere.

By this contrivance, the heat, which would otherwise have been carried off by the steam into the atmosphere and totally lost, was arrested in its flight, and brought back into the boiler, and made to work the second day.

By other contrivances, the smoke also was laid under contribution. After it had passed under the boilers, and just as it was about to escape by the

chimney, it was stopped; and, by being made to pass under a large copper filled with cold water, was deprived of the greater part of the heat it still retained: And thinking it probable that considerable advantages would be derived from drying the wood very thoroughly, and even heating it, before it was made use of for Fuel, the smoke from two of the boilers was made to pass under a plate of iron which formed the bottom of an oven, in which the wood, necessary for the consumption of the kitchen for one day, (having previously been cut into billets of a proper size,) was dried during 24 hours, previous to its being used.

In a smaller kitchen, (adjoining to that I have been describing,) which was constructed merely as a model for imitation, and which was constantly open for the inspection of the Public, five boilers of different sizes, all heated by the same fire, were placed in a semicircular mass of brick-work, and the smoke, after having passed under all these five boilers, was made to heat, at pleasure, either an oven, or water which was contained in a wooden cask set upright upon the brick-work.—A tube of copper, tinned on the outside, which went through the cask, gave a passage to the smoke, and this tube was connected with the bottom of the cask by means of a circular plate of copper through which the tube passed, which plate closed a circular opening in the bottom of the cask somewhat larger in diameter than the tube.

This

This circular plate was nailed to the bottom of the cask, and the joining made water-tight by interposing between the metallic plate and the wood a sheet of pasteboard; and the tube was fastened to the plate with folder. This tube, (which was about 6 inches in diameter,) as soon as it had passed the circular plate, and entered the barrel, branched out into three smaller tubes, each about 4 inches in diameter, which, running parallel to each other through the whole length of the cask, went out of it above, by three different holes in the upper head of the cask, and ended in a canal which led to the chimney.

This tube, by which the smoke passed through the cask, was branched out into a number of branches in order to increase the surface, by which the heat of the smoke was communicated to the water in the cask. The cask was supplied with water from a reservoir placed in the upper part of the building, by means of a leaden pipe of communication from the one to the other; and the machinery was so contrived, that, when any water was drawn out of the cask for use, it was immediately replaced from the reservoir; but as soon as the water in the cask had regained its proper height, the cold water from the reservoir ceased to flow in it.

Nothing more generally excited the surprise and curiosity of those who visited this kitchen, than to see water actually boiled in a wooden cask, and drawn from it, boiling hot, by a brass cock. I have been the more particular in describing the manner

manner in which this was done, as I have reason to think that a contrivance of this kind, or something similar to it, might, in many cases, be applied to useful purposes. No contrivance can possibly be invented by which heat can be communicated to fluids with so little loss; and as wood is not only an excellent non-conductor of heat itself, but may easily be surrounded by confined air, by furs and other like bodies which are known to be useful in confining heat, the loss of heat, by the sides of a containing vessel composed of wood, might be almost entirely prevented.

Why should not the boilers for large salt-works and breweries, and those destined for other similar processes, in which great quantities of water are heated, or evaporated, be constructed of wood, with horizontal tubes of iron or of copper, communicating with the fire-place, and running through them, for the circulation of the smoke?—But this is not the place to enlarge upon this subject; I shall therefore leave it for the present, and return to my kitchens.

To prepare the soup furnished to the Poor from the kitchen of the House of Industry, it was found necessary to keep up the fire near *five hours*, the soup, in order to its being good, requiring to be kept actually boiling above three hours.

The Fuel made use of in this kitchen was dry beech-wood; a cord of which, (or *klafter*, as it is called,) 5 English feet 8 $\frac{2}{3}$ inches long, 5 feet 8 $\frac{1}{2}$ inches high, and 3 feet 1 $\frac{1}{2}$ inches wide, and which

which weighed at an average about 2200 Bavarian pounds, (= 2724 lbs. Avoirdupois,) cost at an average about 5½ florins (= 9s. 6½ d. sterling) in the market.

Of this wood the daily consumption, when soup was provided for 1000 persons, was about 300 lbs. Bavarian weight, or about $\frac{1}{4}$, or more exactly, $\frac{1}{2}$ of a cord or clafter, which cost 43 creutzers, (60 creutzers making a florin,) or about 1s. 3½ d. sterling: And this gives $\frac{1}{11}$ of a creutzer, or $\frac{1}{10}$ of a farthing, for the daily expence for Fuel in cooking for each person.

To make an estimate of the daily expence for Fuel in cooking the same quantity of the same kind of soup in private kitchens, we will suppose these 1000 persons, who were fed from the public kitchen of the House of Industry, to be separated into families of 5 persons each.

This would make just 200 families; and the quantity of wood consumed in the public kitchen daily for feeding 1000 persons, (= 300 lbs.,) being divided among 200 families, gives 1½ lbs. of wood for the daily consumption of each family; and according to this estimate, a cord of wood, weighing 2200 lbs. ought to suffice for cooking for such a family 1466 days, or 4 years and 6 days.

But upon the most careful inquiries relative to the real consumption of Fuel in private families in operations of cookery, as they are now generally performed over an open fire, I find that 5 Bavarian pounds of good peas-soup can hardly be prepared

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at a less expence of Fuel than 15 lbs. of dry beech-wood of the best quality; consequently, a cord of such wood, instead of sufficing for preparing a soup daily for a family of 5 persons for 4 years, would hardly suffice for so long a time as 5 months. 10

And hence it appears, that the consumption of Fuel in the kitchens of private families, is so that consumed in the first kitchen of the House of Industry at Munich, in preparing the same quantity of the same kind of food, (peas-soup,) as 10. to 1. But it must be remembered, that this difference in the quantities of Fuel expended is not occasioned entirely by the difference between the two methods of managing the Fire; for, exclusive of the effect produced by a given arrangement of the machinery, — with the same arrangement, the greater the quantity of food prepared at once, or the larger the boiler, (within certain limits however, as will be seen hereafter,) the less in proportion will be the quantity of Fuel required; — and the saving of Fuel which arises from cooking upon a large scale is very considerable. But I shall take occasion to treat this part of my subject more fully elsewhere. 10

The kitchen in the House of Industry was finished in the beginning of the year 1790. And much about the same time, two other public kitchens upon a large scale were erected at Munich, under my directions; namely, the kitchen belonging to

• Afterwards, on altering the kitchen of the House of Industry, and fitting it up on better principles, the Economy of Fuel was carried still farther, as will be seen in the sequel of this Essay. 20

the Military Academy, and that belonging to the Military Hall (as it is called) in the English garden, in which building near 200 military officers mess'd daily during the annual encampments,—for which purpose this building was erected.

There is likewise in the garden, (which is 6 English miles in circumference,) an inn—a farm-house, and a large dairy; and these establishments gave me an opportunity of constructing no less than four other kitchens;—namely, two for the inn, one for the farm-house, and one for the use of the dairy. And the uses for which these different kitchens were designed, and to which they were applied, were so various, as not only to include almost every process of cookery, but also to afford opportunities of performing the same operations upon very different scales, and consequently of making many interesting Experiments relative to the Management of Heat, and the Economy of Fuel.

That I did not neglect these opportunities of pursuing, with effect, a subject which had long engaged my attention, and to which I was much attached, will readily be believed by those who know what ardour a curious subject of philosophical investigation is capable of inspiring in an inquisitive mind.

As the Experiments I have made, or caused to be made, in the different establishments before mentioned, during the six or seven years that they have existed, are extremely numerous; it would take up too much time to give an account of them in detail; I shall therefore content myself with merely
noticing

noticing the general results of them, and mentioning more particularly only such of them as appear to me to be most important. And in regard to the peculiar construction of the different kitchens above mentioned, as most of them have undergone many alterations, and as no one of them remains exactly in the same state in which it was first constructed, I do not think it necessary to be very particular in my account of them; I shall occasionally mention the principles on which they were constructed, and the faults I discovered in them; but when I shall come to speak of those improvements which have stood the test of actual experience, and which I can recommend as being worthy of imitation, I shall take care to be very exact and particular in my descriptions.

It will not be found very difficult, I fancy, from what has been said, to form a pretty just idea of the construction of the kitchen in the House of Industry above described, even without the help of a plan or drawing of it. That in the Military Academy was constructed upon a different principle: Instead of heating all the boilers from one and the same fireplace, almost every boiler had its own separate fireplace; and though the boilers were all furnished with double covers, similar to those made use of in the kitchen of the House of Industry, yet there was no attempt made to recover the heat carried off by the steam, but it was suffered to escape without hindrance into the atmosphere; it having been found, by the experiments made in the kitchen of the
House

House of Industry, that when the Fire is properly managed, that is to say, when the heat is but just sufficient to keep the liquid boiling hot, or *very gently boiling*, the quantity of steam generated is inconsiderable, and the heat carried off by it not worth the trouble of saving. Each fire-place was furnished with an iron grate, upon which the wood was burnt, and the opening into the fire, as well as that which communicated with the ash-pit, had in each its separate iron door.

Finding afterwards that the iron door which closed the opening by which the wood was introduced into the fire-place, was much heated, and consequently that it caused a considerable loss of heat by communicating it to the cold atmosphere with which it was in contact; in order to remedy this evil without incurring the expence of double doors, the iron door was removed, and in its stead was placed a hollow cylinder, or rather truncated cone, of burnt clay or common earthen-ware, which cone was 4 inches long, 6 inches in diameter internally, and 8 inches in diameter externally, at its larger end or base; and $5\frac{1}{2}$ inches in diameter internally, and $7\frac{1}{2}$ inches in diameter externally, at its smaller end: And being firmly fixed, with its axis in an horizontal position, and its larger end or base outwards, in the middle of the opening leading to the fire-place, and being well united with the solid brick-work by means of mortar, the cavity of this cone formed the opening by which the wood was introduced into the fire-place. This
cavity

cavity being closed with a fit stopper of earthenware, as earthen-ware is a non-conductor of heat, or as heat cannot pass through it but with great difficulty, and very slowly, the external surface of this cone and its stopper were never much heated, consequently the quantity of heat they could communicate to the atmosphere was but very trifling. This contrivance was afterwards rendered much more simple, by substituting, instead of the hollow cone, a tile, 10 inches square, and about $2\frac{1}{2}$ inches thick, with a conical hole in its center, 6 inches in diameter externally, and $5\frac{1}{4}$ inches in diameter within, provided with a fit baked earthen stopper. (See the Figures, N^o 6, 7, and 8. Plate I.)

A perforated square tile is preferable to a hollow cylinder for forming a passage into the fire-place, not only because it is cheaper, stronger, and more durable, but also because it may, on account of its form, be more easily and more firmly fixed in its place, and united with the rest of the brick-work.

If proper moulds be provided for forming these perforated tiles and their stoppers, they may be afforded for a mere trifle. In Munich they are made of the very best earth, by the Elector's potter, and they cost no more than 24 creutzers, or something less than 9d. sterling, for a tile with its stopper. I had several made of sand-stone by a stone-cutter, but they cost me 1 florin and 30 creutzers, or about 2s. 9d. sterling each.

Though those made of stone answered perfectly well, yet I found them not better than those made
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of earthen-ware; and as these last are much cheaper, and I believe equally durable, they ought certainly to be preferred. That the stopper may be made to fit with accuracy the hole it is intended to close, (which is necessary, as will be seen hereafter,) they may be ground together with fine sand moistened with water.

Sensible, from the beginning, of the great importance of being absolutely master of the air which is admitted into the fire-place to feed the Fire, so as to be able to admit more or less at pleasure, or to exclude it entirely; I took care, in all my fire-places, to close very exactly the passage into the ash-pit by a door carefully fitted to its frame, the air being admitted through a semicircular opening furnished with a register in the middle of this door. This contrivance (which admits of no further improvement) is indispensably necessary in all well-constructed fire-places, great or small. (See the Figures from Fig. 9 to Fig. 16. Plate II.)

Having occasion, in the course of my arrangements, to make use of a great number of boilers, and often of several boilers of the same dimensions, I availed myself of that circumstance to determine, by actual experiments, the best form for boilers, or that form which, with any given capacity, shall be best adapted for saving Fuel.

Two or more boilers of the same capacity, but of different forms, constructed of sheet copper of the same thickness, were placed in closed fire-places, constructed as nearly as possible upon the same principles,

principles, and were used for a length of time in the same culinary processes; and the quantity of Fuel consumed by each being noted, the comparative advantages of their different forms were ascertained. Some of these boilers were made deep and narrow;—others wide and shallow;—there were some with flat bottoms; others of a globular form; and others again with their bottoms drawn inward like the bottom of a common glass bottle. The results of these inquiries were very curious, and led me to a most interesting discovery:—They taught me not only what forms are best for boilers; but also (what is still more interesting) *why* one form is preferable to another:—They gave me much new light with respect to the *manner* in which flame and hot vapour part with their heat; and suggested to me the idea of a very important improvement in the internal construction of fire-places, which I have since put in practice with great success.

But in order to be able to explain this matter in a clear and satisfactory manner, and to render it easier to be understood by those who have not been much conversant in inquiries of this kind, it will be necessary to go back a little, and to treat the subject under consideration in a more regular and scientific manner.

Though it was not my intention originally to write an elementary treatise on Heat, yet, as the first or fundamental principles of that science are necessary to be known, in order to establish upon solid

grounds the practical rules and directions relative to the management of Heat which will hereafter be recommended, it will not, I trust, be deemed either improper or superfluous, to take a more extensive view of the subject, and to treat it methodically, and at some length.

I have perhaps already exposed myself to criticism, by paying so little attention to method in this Essay, as to postpone so long the investigation of the elementary principles of the science I have undertaken to treat.—It may be thought that the part of the subject I am now about to consider should have preceded all other investigation;—that instead of occupying the middle of my book, it ought to have been discussed in the Introduction, or at least to have been treated in the beginning of the first chapter:—But if I have been guilty of a fault in the arrangement of my subject, it has arisen, not from inattention, but from an error of judgment. Desirous rather of writing an *useful book*, than of being the Author of a *splendid performance*, I have not scrupled to transgress the established rules of elegant composition in all cases where I thought it would contribute to my main design, *public utility*:—And well aware that my book, in order to its being really useful, must be read by many who have neither time nor patience to labour through an elementary treatise upon so abstruse a subject, I have endeavoured to *decoy* my reader into the situation in which I wish him to be placed, in order to his having a complete view of the prospect I have pre-

pared for him, rather than to force him into it. If I have used art in doing this, he must forgive me; my design was not only innocent, but such as ought to entitle me to his thanks and to his esteem. I wished to entice him on as far as possible, without letting him perceive the difficulties of the road; and now that we have come on together so far, and are so near our journey's end, I hope and trust that he will not leave me.—To proceed therefore—

C H A P. II.

Of the GENERATION OF HEAT in the COMBUSTION OF FUEL.—Without knowing what Heat really is, the Laws of its Action may be investigated.—Probability that the Heat generated in the Combustion of Fuel is furnished by the Air, and not by the Fuel.—Effects of blowing a Fire explained.—Of Fire-places in which the Fire is made to blow itself.—Of Air-furnaces.—These Fire-places illustrated by a Lamp on Argand's Principle.—Great Importance of being able to regulate the Quantity of Air which enters a closed Fire-place.—Utility of Dampers in the Chimnies of closed Fire-places.—General Rules and Directions for constructing closed Fire-places; with a full Explanation of the Principles on which these Rules are founded.

WITHOUT entering into those abstruse and most difficult investigations respecting the Nature of FIRE, which have employed the attention and divided the opinions of speculative philosophers in all ages;—without even attempting to determine whether there be such a thing as an igneous fluid, or not;—whether what we call *Heat* be occasioned by the accumulation, or by the increased action of such a fluid;—or whether it arises merely from an increased motion in the component particles

ticles of the body heated, or of some elastic fluid by which those particles are supposed to be surrounded, and upon which they are supposed to act, or by which they are supposed to be acted upon:—In short, without bewildering myself and my reader in this endless labyrinth of darkness and uncertainty, I shall confine my inquiries to objects more useful, and which are clearly within the reach of human investigation;—namely, the discovery of the sensible properties of Heat, and of the most advantageous methods of generating it, and of directing it with certainty and effect in those various processes in which it is employed in the economy of human life.

Though I do not undertake to determine *what Heat really is*, nor even to offer any opinions or conjectures relative to that subject; yet as Heat is evidently something capable of being excited or generated, increased or accumulated, measured and transferred from one body to another; in treating the subject, I shall speak of it as being *generated, confined, directed, dispersed, &c.*; it being necessary to use these terms in order to make myself understood.

Though it is not known exactly *how much* Heat it is possible to produce in the combustion of any given quantity of any given kind of Fuel, yet it is more than probable that the quantity depends in a great measure on the Management of the Fire. It is likewise probable—I might say certain—that the Heat produced is furnished, not merely by the Fuel,

Fuel, but in a great measure, if not entirely, by the *air* by which the Fire is fed and supported. It is well known that air is necessary to combustion; it is likewise known that the pure part of common atmospheric air, or that part of it (amounting to about $\frac{1}{3}$ of its whole volume) which alone is capable of supporting the combustion of inflammable bodies, undergoes a remarkable change, or is actually *decomposed* in that process; and as in this decomposition of pure air a great quantity of heat is known to be set loose, or to become redundant, it has been supposed by many, (and with much appearance of probability,) that by far the greater part, if not all the heat produced in the combustion of inflammable bodies, is derived from this source.

But whether it be the air or the Fuel which furnishes the heat, it seems to be quite certain that the quantity furnished depends much upon *the Management of the Fire*, and that the quantity is greater as the combustion or decomposition of the Fuel is more complete. In all probability, the decomposition of the air keeps pace with the decomposition of the Fuel.

It is well known that the consumption of Fuel is much accelerated, and the intensity of the heat augmented, by causing the air by which the combustion is excited, to flow into the fire-place in a continued stream, and with a certain degree of velocity. Hence, blowing a fire, when the current of air is properly directed, and when it is not too strong, serves to accelerate the combustion, and to

increase the heat; but when the blast is improperly directed, it will rather serve to derange and to impede the combustion than to forward it; and when it is too strong, it will blow the Fire quite out, or totally extinguish it. There is no fire, however intense, but may be blown out by a blast of air, provided it be sufficiently strong, and that as infallibly as by a stream of cold water. Even gun-powder, the most inflammable perhaps of known substances, may be actually on fire at its surface, and yet the Fire may be blown out and extinguished before the grain of powder has had time to be entirely consumed.

This fact, however extraordinary and incredible it may appear, I have proved by the most unexceptionable and conclusive experiments.

Fire-places may be so constructed that the Fire may be made to blow itself, or—which is the same thing—to cause a current of air to flow into the Fire: And this is an object to which the greatest attention ought to be paid in the construction of all fire-places where it is not intended to make use of an artificial blast from bellows for blowing the Fire. Furnaces constructed upon this principle have been called *air-furnaces*; but every fire-place, and particularly every closed fire-place, ought to be an air-furnace, and that even were it intended to serve only for the smallest saucepan, otherwise it cannot be perfect.

An Argand's lamp is a fire-place upon this construction; for the glass tube which surrounds the wick

wick (and which distinguishes this lamp from all others) serves merely as a blower. The circular form of the wick is not essential; for by applying a flattened glass tube as a blower to a lamp with a flat or riband wick, it may be made to give as much light as an Argand's lamp; or at least quite as much in proportion to the size of the wick, and to the quantity of oil consumed, as I have found by actual experiment.

But it is not the light alone that is increased in consequence of the application of these blowers;—the heat also is rendered much more intense;—and as the heat of any fire may be increased by a similar contrivance, on that account it is that I have had recourse to these lamps to assist me in explaining the subject under consideration. In these lamps the fire-place is closed on all sides, and the current of air which feeds the Fire rises up perpendicularly from below the fire-place into the Fire. By surrounding the Fire on all sides by a wall, the cold atmosphere is prevented from rushing in laterally from all quarters to supply the place of the heated air or vapour, which, in consequence of its increased elasticity from the heat, continually rises from the Fire, and this causes the current of air below (the only quarters from which it can with advantage flow into the Fire) to be very strong.

But in order that a fire-place may be perfect, it should be so contrived that the combustion of the Fuel, and the generation of the heat, may occa-

sionally be accelerated or retarded, *without adding to or diminishing the quantity of Fuel*; and, when the fire-place is closed, this may easily be done by means of a *register* in the door which closes the passage leading to the ash-pit;—for, as the rapidity of the combustion depends upon the quantity of air by which the Fire is fed, by opening the register more or less, more or less air will be admitted into the fire-place, and consequently more or less Fuel will be consumed, and more or less heat generated in any given time, though the quantity of Fuel in the fire-place be actually much greater than what otherwise would be sufficient.—Fig. 9. shews the form of the register I commonly use for this purpose.

In order that this register may produce its proper effect, a valve, or a *damper*, as it is commonly called, should be placed in the chimney or canal by which the smoke is carried off; which damper should be opened more or less, as the quantity of air is greater or less which is admitted into the fire-place. This register and this damper will be found very useful in another respect, and that is, in putting out the Fire when there is no longer an occasion for it; for, upon closing them both entirely, the Fire will be immediately extinguished, and the half-consumed Fuel, instead of being suffered to burn out to no purpose, will be saved.

Nearly the same effects as are produced by a damper may be produced without one, by causing the smoke, after it has quitted the fire-place, to descend

descend several feet below the level of the grate on which the Fuel is burned, before it is permitted to go up the chimney.

There is another circumstance of much importance which must be attended to in the construction of fire-places; and that is, the proper disposition of the Fuel; for in order that the combustion may go on well, it is necessary, not only that the Fuel be in its proper place, but also that it be properly disposed; —that is to say, that the solid parts of the Fuel be of a just size, and that they be not placed too near each other, so as to prevent the free passage of the air between them, nor too far asunder; and if the fire-place can be so contrived, that solid pieces of the inflamed Fuel, as they go on to be diminished in size as they burn, may naturally fall together in the center of the fire-place without any assistance, it will be a great improvement, as I have found by experience. This may be done, in small fire-places, (and in these it is more particularly necessary,) by burning the Fuel upon a grate in the form of a segment of a hollow sphere, or of a dish. (See the Figures 3 and 4. Plate I.) All those I now use, except it be for fire-places which are very large indeed, are of this form; and where wood is made use of for Fuel, it is cut into small billets from 4 to 6 inches in length. Instead of a grate of iron, I have lately introduced grates, or rather hollow dishes or pans, of earthen ware, perforated with a great number of holes for giving a passage to the air.

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These perforated earthen pans, which are made very thick and strong, are incomparably cheaper than iron grates; and judging from the experience I have had of them, I am inclined to think they answer even better than the grates; indeed it appears to me not difficult to assign a reason why they ought to be better.

For large fire-places I have sometimes used grates, the bars of which were common bricks placed edgewise, and these have been found to answer very well.

As only *that part of the air* which, entering the fire-place in a proper manner, and in a just quantity, and coming into actual contact with the burning Fuel, is *decomposed*, contributes to the generation of heat; it is evident that all the air that finds its way into the fire-place, *and out of it again*, without being decomposed, is a thief;—that it not only *contributes nothing* to the heat, but being itself heated at the expence of the Fire, and going off *hot* into the atmosphere by the chimney, occasions an actual loss of heat; and this loss is often very considerable, and the prevention of it is such an object, that too much attention cannot be paid to it in the construction of fire-places.

When the fire-place is closed on all sides by a wall, and when the opening by which the Fuel is introduced is kept closed, no air can press in laterally upon the Fire; but yet, when the grate is larger than the heap of burning Fuel, which must often be the case, a great quantity of air may insinuate

sinuate itself by the sides of the grate into the fire-place, without going through the Fire: But when, instead of an iron-grate, a perforated hollow earthen pan is used, by making the bottom of the pan of a certain thickness, 2, 3, or 4 inches, for instance, and making all the air-holes point to one common center, (to the focus or center of the Fire,) this fugitive entrance of cold air into the fire-place will, in a great measure, be prevented.

This evil may likewise be prevented when circular hollow iron grates are used, by narrowing the fire-place immediately under the grate, in the form of an inverted, truncated, hollow cone, the opening or diameter of which above being equal to the internal diameter of the circular rim of the grate, and that below (by which the air rises to enter the fire-place) about *one-third* of that diameter. (See the Figure 5. Plate I.) This opening below, through which the air rises, must be immediately under the center of the grate, and as near to it as possible; care must be taken, however, that a small space be left between the outside or underside of the iron bars which form the hollow grate, and the inside surface of this inverted hollow cone, in order that the ashes may slide down into the ash-pit.

As to the form and size of the ash-pit, these are matters of perfect indifference, provided, however, that it be large enough to give a free passage to the air necessary for feeding the Fire, and that the only passage into it, by which air can enter, is closed by a good door furnished with a register.

The necessity of being completely master of the passage, by which the air enters the fire-place, has already been sufficiently explained.

It is perhaps unnecessary for me to observe, that where perforated earthen pans are used instead of iron grates, the air-holes in the pans ought to be rather smaller above than below, in order that they may not be choaked up by the small pieces of coal, and the ashes which occasionally fall through them into the ash-pit.

One great advantage attending fire-places on the construction here proposed, is, that they serve equally well for every kind of Fuel. Wood, pit-coal, charcoal, turf, &c. may indifferently be used, and all of them with the same facility, and with the same advantages; or any two, or more, of these different kinds of Fuel, may be used at the same time without the smallest inconvenience;—or the Fire having been lighted with dry wood, or any other very inflammable material, the heat may afterwards be kept up by cheaper or more ordinary Fuel of a more difficult and slow combustion.—Some kinds of Fuel will perhaps be found most advantageous for making the pot boil, and others for keeping it boiling; and a very considerable saving will probably be found to result from paying due attention to this circumstance. When the fire-place is so contrived as to serve equally well for all kinds of Fuel, this may be done without the least difficulty or trouble.

I have just shown, that narrowing that part of the fire-place which lies below the grate, serves to make the air enter the fire in a more advantageous manner. This construction has another advantage, perhaps still more important; the heat which is projected downwards through the openings between the bars of the grate, instead of being permitted to escape into the ash-pit, (where it would be lost,) striking against the sides of this inverted hollow cone, it is there stopped, and afterwards rises into the fire-place again with the current of air which feeds the Fire, or it is immediately reflected by this conical surface, and, after two or three bounds from side to side, is thrown up against the bottom of the boiler.

But in order to be able to form a clear and distinct idea upon this subject, it is necessary to examine with care all the circumstances attending the generation of heat in the combustion of inflammable bodies, and to see in what manner, or under what form, the heat generated manifests itself, and how it may be collected, accumulated, confined, and directed.

This opens a wide field for philosophical inquiry; but as these investigations are not only curious and entertaining, but also useful and important in a high degree, I trust my reader will pardon me for requesting his particular attention while I endeavour to do justice to this most interesting, but, at the same time, most abstruse and most difficult part of the subject I have undertaken to treat.

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20 The heat generated in the combustion of Fuel manifests itself in two ways; namely, in the hot vapour which rises from the Fire, with which it may be said to be *combined*, and in the calorific rays which are thrown off from the Fire in all directions.—These rays may, with greater propriety, be said to be *calorific*, or *capable of generating heat*, in any body by which they are *stopped*, than to be called hot; for when they pass freely through any medium, (as through a mass of air, for instance,) they are not found to communicate any heat whatever to such medium; neither do they appear to excite any considerable degree of heat in bodies from whose surfaces they are reflected; and in these respects they bear a manifest resemblance to the rays emitted by the sun.

21 What proportion this *radiant heat* (if I may be allowed to use so inaccurate an expression) bears to that which goes off from burning bodies in the smoke and heated vapour, is not exactly known; it is certain, however, that the quantity of heat which goes off in the heated elastic fluids, visible and invisible, which rise from a Fire, is much greater than that which all the calorific rays united would be capable of producing. But though the quantity of *radiant heat* is less than that existing in the hot vapour, (and which, for the sake of distinction, may be called *combined heat*,) the former is still much too considerable to be neglected.

22 That the heat generated, or excited, by the calorific rays which proceed from burning bodies, is in fact

fact considerable, is evident from the heat which is felt in a room warmed by a chimney Fire; for as all the heat, combined with the smoke and hot vapour, goes up the chimney, it is certain that the increase of heat in the room, occasioned by the Fire, is entirely owing to the calorific rays thrown into it from the burning Fuel.

The activity of these rays may be shown in various ways, but in no way in a more striking manner than by the following simple Experiment: When the Fire burns bright upon the hearth, let the arm be extended in a straight line towards the center of the Fire, with the hand open, and all the fingers extended and pointing to the Fire. If the hand is not nearer the Fire than the distance of two or three yards, except the Fire be very large indeed, the heat will scarcely be perceptible; but if, without moving the arm, the wrist be bent upwards so as to present the inside or flat of the hand perpendicular to the Fire, the heat will not only be very sensibly felt, but, if the Fire be large, and if it burns clear and bright, it will be found to be so intense as to be quite insupportable.

It is not, however, burning bodies alone that emit calorific rays. All bodies,—those which are fixed and incombustible, as well as those which are inflammable,—fluids as well as solids,—are found to throw off these rays in great abundance, as soon as they are heated to that degree which is necessary to their becoming luminous in the dark, or till they are red-hot.

Bodies

Bodies even which are heated to a less degree than that which is necessary to their emitting visible light, send off calorific rays in all directions. This is a matter of fact, which has been proved by experiment. Do all bodies, at all temperatures,—freezing mercury as well as melting iron,—continually emit these rays in greater or less quantities, or with greater or less velocities?—Are bodies cooled in consequence of their emitting these rays?—Do these calorific rays always generate heat, even when the body, by which they are stopped or absorbed, is hotter than that from which the rays proceeded?—But I forget that I promised not to involve myself in abstruse speculation.—To return then:—Whatever may be the nature of the rays emitted by burning Fuel, as one of their known properties is to generate heat, they ought certainly to be very particularly attended to in every arrangement in which the Economy of Heat, or of Fuel, is a principal object in view.

As these calorific rays generate heat in the body by which they are stopped or absorbed, and not in the medium through which they pass, it is necessary to dispose those bodies which are designed for stopping them, in such a manner that they may easily and necessarily communicate the heat they thus acquire to the body upon which it is intended that it should operate.

The closed fire-places which I have recommended, and which will hereafter be more particularly described, will answer this purpose completely.

The

The Fire being closed in these fire-places, on every side, as well below the grate as laterally, and in short every where, except where the bottom of the boiler presents itself to the Fire, none of these rays can possibly escape; and as the materials of which the fire-place is constructed, (bricks and mortar,) are bad conductors of heat, but a small part of the heat generated in the combustion of the Fuel will be absorbed and transmitted by them into the interior parts of the wall, there to be dispersed and lost. But the confining of heat is a matter of sufficient importance to deserve being treated in a separate Chapter.

C H A P. III.

Of the Means of CONFINING HEAT, and DIRECTING ITS OPERATIONS.—Of Conductors and Non-conductors of Heat.—Common Atmospheric Air a good Non-conductor of Heat, and may be employed with great Advantage for confining it—is employed by Nature for that Purpose, in many Instances—is the principal Cause of the Warmth of Natural and Artificial Clothing—is the sole Cause of the Warmth of Double Windows.—Great Utility of Double Windows and Double Walls—they are equally useful in Hot Countries as in Cold.—ALL ELASTIC FLUIDS Non-conductors of Heat.—STEAM proved by Experiment to be a Non-conductor of Heat.—FLAME is also a Non-conductor of Heat.

THAT HEAT passes more freely through some bodies than through others, is a fact well known; but the cause of this difference in the conducting powers of bodies, with respect to Heat, has not yet been discovered.

The utility of giving a wooden handle to a tea-pot or coffee-pot of metal, or of covering its metallic handle with leather, or with wood, is well known: But the difference in the conducting powers of various bodies with regard to Heat, may be shown by a great number of very simple experiments;—such

as are in the power of every one to make at all times and in all places, and almost without either trouble or expence.

If an iron nail and a pin of wood, of the same form and dimensions, be held successively in the flame of a candle, the difference in the conducting powers of the metal and of wood will manifest itself in a manner in which there will be no room left for doubt. As soon as the end of the nail, which is exposed in the flame of the candle, begins to be heated, the other end of it will grow so hot as to render it impossible to hold it in the hand without being burnt; but the wood may be held any length of time in the same situation without the least inconvenience; and, even after it has taken fire, it may be held till it is almost entirely consumed; for the uninflamed wood will not grow hot, and, till the flame actually comes in contact with the fingers, they will not be burnt. If a small slip or tube of glass be held in the flame of the candle in the same manner, the end of the glass by which it is held will be found to be more heated than the wood, but incomparably less so than the pin or nail of metal;—and among all the various bodies that can be tried in this manner, no two of them will be found to give a passage to Heat through their substances with exactly the same degree of facility*.

To

* To show the relative conducting power of the different metals, Doctor Ingenhouz contrived a very pretty experiment. He took

To confine Heat is nothing more than to prevent its escape out of the hot body in which it exists, and in which it is required to be retained; and this can only be done by surrounding the hot body by some covering composed of a substance through which Heat cannot pass, or through which it passes with great difficulty. If a covering could be found perfectly impervious to Heat, there is reason to believe that a hot body, completely surrounded by it, would remain hot for ever; but we are acquainted with no such substance; nor is it probable that any such exists.

Those bodies in which Heat passes freely or rapidly, are called *Conductors* of Heat; those in which it makes its way with great difficulty, or very slowly, *Non-conductors*, or bad *Conductors* of Heat. The epithets, good, bad, indifferent, excellent, &c. are applied indifferently to *conductors* and to *non-conductors*. A good conductor, for instance, is one in which Heat passes very freely; a good non-conductor is one in which it passes with great difficulty; and an indifferent conductor may likewise be called, without any impropriety, an indifferent non-conductor.

equal cylinders of the different metals, (being straight pieces of stout wire, drawn through the same hole, and of the same length,) and, dipping them into melted wax, covered them with a thin coating of the wax. He then held one end of each of these cylinders in boiling water, and observed how far the coating of wax was melted by the Heat communicated through the metal, and with what celerity the Heat passed.

Those

Those bodies which are the worst conductors, or rather the best non-conductors of Heat, are best adapted for forming coverings for confining Heat.

All the metals are remarkably good conductors of Heat;—wood, and in general all light, dry, and spongy bodies, are non-conductors: Glass, though a very hard and compact body, is a non-conductor. Mercury, water, and liquids of all kinds, are conductors; but air, and in general all elastic fluids, *steam* even not excepted, are non-conductors.

Some experiments which I have lately made, and which have not yet been published, have induced me to suspect, that *water*, mercury, and all other non-elastic fluids, do not permit Heat to pass through them from particle to particle, as it undoubtedly passes through solid bodies, but that their apparent conducting powers depend essentially upon the extreme mobility of their parts; in short, that they rather *transport* Heat than allow it a passage. But I will not anticipate a subject which I propose to treat more fully at some future period.

The conducting power of any solid body in one solid mass, is much greater than that of the same body reduced to a powder, or divided into many smaller pieces: An iron bar, or an iron plate, for instance, is a much better conductor of Heat than iron filings; and saw-dust is a better non-conductor than wood. Dry wood-ashes is a better non-conductor than either; and very dry charcoal reduced to a fine powder is one of the best non-conductors

known; and as charcoal is perfectly incombustible when confined in a space where fresh air can have no access, it is admirably well calculated for forming a barrier for confining Heat, where the Heat to be confined is intense.

But among all the various substances of which coverings may be formed for confining Heat, none can be employed with greater advantage than common atmospheric air. It is what Nature employs for that purpose; and we cannot do better than to imitate her.

The warmth of the wool and fur of beasts, and of the feathers of birds, is undoubtedly owing to the air in their interstices; which air, being strongly attracted by these substances, is confined, and forms a barrier which not only prevents the cold winds from approaching the body of the animal, but which opposes an almost insurmountable obstacle to the escape of the Heat of the animal into the atmosphere. And in the same manner the air in snow serves to preserve the Heat of the earth in winter. The warmth of all kinds of artificial clothing may be shown to depend on the same cause; and were this circumstance more generally known, and more attended to, very important improvements in the Management of Heat could not fail to result from it. A great part of our lives is spent in guarding ourselves against the extremes of heat and of cold, and in operations in which the use of Fire is indispensable; and yet how little progress has been made in that most useful and most

most important of the arts,—the Management of Heat!

Double windows have been in use many years in most of the northern parts of Europe, and their great utility, in rendering the houses furnished with them warm and comfortable in winter, is universally acknowledged,—but I have never heard that any body has thought of employing them in hot countries to keep their apartments cool in summer;—yet how easy and natural is this application of so simple and so useful an invention!—If a double window can prevent the Heat which is *in* a room from passing *out of it*, one would imagine it could require no great effort of genius to discover that it would be equally efficacious for preventing the Heat *without* from coming *in*. But natural as this conclusion may appear, I believe it has never yet occurred to any body; at least, I am quite certain that I have never seen a double window either in Italy, or in any other hot country I have had occasion to visit.

But the utility of double windows and double walls, in hot as well as in cold countries, is a matter of so much importance that I shall take occasion to treat it more fully in another place. In the mean time, I shall only observe here, that it is the *confined air* shut up between the two windows, and not the double glass plates, that renders the passage of Heat through them so difficult. Were it owing to the increased thickness of the glass, a single pane of glass twice as thick would answer the same pur-

pose; but the increased thickness of the glass of which a window is formed, is not found to have any sensible effect in rendering a room warmer.

But air is not only a non-conductor of Heat, but its non-conducting power may be greatly increased. To be able to form a just idea of the manner in which air may be rendered a worse conductor of Heat, or, which is the same thing, a better non-conductor of it than it is in its natural unconfined state, it will be necessary to consider *the manner* in which Heat passes through air. Now it appears, from the result of a number of Experiments which I made with a view to the investigation of this subject, and which are published in a Paper read before the Royal Society *, that though the particles of air, *each particle for itself*, can receive Heat from *other bodies*, or communicate it to them, yet there is no communication of Heat *between one particle of air and another particle of air*. And from hence it follows, that though air may, and certainly does, *carry off* Heat, and *transport it* from one place, or from one body to another, yet a mass of air in a quiescent state, or with all its particles at rest, *could it remain in this state*,—would be totally impervious to Heat; or such a mass of air would be a perfect non-conductor.

Now if Heat passes in a mass of air merely in consequence of the motion it occasions in that air,—if it is *transported*,—*not suffered to pass*,—in that case,

* See the Philosophical Transactions, 1793.

it is clear that whatever can obstruct and impede the internal motion of the air, must tend to diminish its conducting power: And this I have found to be the case in fact. I found that a certain quantity of Heat which was able to make its way through a wall, or rather a sheet of confined air $\frac{1}{2}$ an inch thick in $9\frac{1}{2}$ minutes, required $21\frac{1}{2}$ minutes to make its way through the same wall, when the internal motion of this air was impeded by mixing with it $\frac{1}{3}$ part of its bulk of eider-down,—of very fine fur, or of fine silk, as spun by the worm.

But in mixing bodies with air, in order to impede its internal motion, and render it more fit for confining Heat, such bodies only must be chosen as are themselves non-conductors of Heat, otherwise they will do more harm than good, as I have found by experience. When, instead of making use of eider-down, fur, or fine silk, for impeding the internal motion of the confined air, I used an equal volume of exceedingly fine silver-wire flatted, (being the ravellings of gold or silver lace,) the passage of the Heat through the barrier, so far from being impeded, was remarkably facilitated by this addition; the Heat passing through this compound of air and fine threads of metal much sooner than it would have made its way through the air alone.

Another circumstance to be attended to in the choice of a substance to be mixed with air, in order to form a covering or barrier for confining Heat, is the fineness or subtility of its parts; for the finer they are, the greater will be their surface in proportion

tion to their solidity, and the more will they impede the motions of the particles of the air. Coarse horse-hair would be found to answer much worse for this purpose than the fine fur of a beaver, though it is not probable that there is any essential difference in the chymical properties of those two kinds of hair.

But it is not only the fineness of the parts of a substance, and its being a non-conductor, which render it proper to be employed in the formation of covering to confine Heat;—there is still another property, more occult, which seems to have great influence in rendering some substances better fitted for this use than others; and this is a certain attraction which subsists between certain bodies and air. The obstinacy with which air adheres to the fine fur of beasts and to the feathers of birds, is well known; and it may easily be proved that this attraction must assist very powerfully in preventing the motion of the air concealed in the interstices of those substances, and consequently in impeding the passage of Heat through them.

Perhaps there may be another still more hidden cause which renders one substance better than another for confining Heat. I have shown by a direct and unexceptionable Experiment, that Heat can pass through the Torricellian vacuum*, though with rather more difficulty than in air (the con-

* See my Experiments on Heat, published in the Philosophical Transactions, vol. lxxvi.

ducting

ducting power of air being to that of a Torricellian vacuum as 1000 to 604, or as 10 to 6, very nearly); but if Heat can pass where there is no air, it must in that case pass by a medium more subtile than air;—a medium which most probably pervades all solid bodies with the greatest facility, and which must certainly pervade either the glass or the mercury employed in making a Torricellian vacuum.

Now, if there exists a medium more subtile than air, by which Heat may be conducted, is it not possible that there may exist a certain affinity between that medium and sensible bodies? a certain attraction or cohesion by means of which bodies in general, or some kinds of bodies in particular, may, some how or other, impede this medium in its operations in conducting or transporting Heat from one place to another?—It appeared from the result of several of my Experiments, of which I have given an account in detail in my paper before mentioned, published in the year 1786 in the LXXVth Vol. of the Philosophical Transactions, that the conducting power of a Torricellian vacuum is to that of air as 604 to 1000:—but I found by a subsequent Experiment, (see my second Paper on Heat, published in the Philosophical Transactions for the year 1792,)—that 55 parts in bulk of air, with 1 part of fine raw silk, formed a covering for confining Heat, the conducting power of which was to that of air as 576 to 1284; or as 448 to 1000. Now, from the result of this last-mentioned Experiment,
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it should seem that the introduction into the space through which the Heat passed, of so small a quantity of raw silk as $\frac{1}{36}$ part of the volume, or capacity of that space, rendered that space (which now contained 55 parts of air and 1 part of silk) more impervious to Heat than even a Torricellian vacuum.—The silk must therefore not only have completely destroyed the conducting power of the air, but must also at the same time have very sensibly impaired that of the ethereal fluid which probably occupies the interstices of air, and which serves to conduct Heat through a Torricellian vacuum: For a Torricellian vacuum was a better conductor of Heat, than this medium, in the proportion of 604 to 448. But I forbear to enlarge upon this subject, being sensible of the danger of reasoning upon the properties of a fluid whose existence even is doubtful; and feeling that our knowledge of the nature of Heat, and of the manner in which it is communicated from one body to another, is much too imperfect and obscure to enable us to pursue these speculations with any prospect of success or advantage.

Whatever may be the *manner* in which Heat is communicated from one body to another, I think it has been sufficiently proved that it passes with great difficulty through confined air; and the knowledge of this fact is very important, as it enables us to take our measures with certainty and with facility for confining Heat, and directing its operations to useful purposes.

But

But atmospheric air is not the only non-conductor of Heat. All kinds of air, artificial as well as natural, and in general all elastic fluids, *steam not excepted*, seem to possess this property in as high a degree of perfection as atmospheric air.

That steam is not a conductor of Heat, I proved by the following Experiment: A large globular bottle being provided, of very thin and very transparent glass, with a narrow neck, and its bottom drawn inward so as to form a hollow hemisphere about 6 inches in diameter; this bottle, which was about 8 inches in diameter externally, being filled with cold water, was placed in a shallow dish, or rather plate, about 10 inches in diameter, with a flat bottom formed of very thin sheet brass, and raised upon a tripod, and which contained a small quantity (about $\frac{2}{10}$ of an inch in depth) of water; a spirit lamp being then placed under the middle of this plate, in a very few minutes the water in the plate began to boil, and the hollow formed by the bottom of the bottle was filled with clouds of steam, which, after circulating in it with surprising rapidity 4 or 5 minutes, and after forcing out a good deal of air from under the bottle, began gradually to clear up. At the end of 8 or 10 minutes (when, as I supposed, the air remaining with the steam in the hollow cavity formed by the bottom of the bottle, had acquired nearly the same temperature as that of the steam) these clouds totally disappeared; and, though the water continued to boil with the utmost violence, the contents of this hollow

low

low cavity became so perfectly invifible, and fo little appearance was there of steam, that, had it not been for the freams of water which were continually running down its fides, I fhould almoft have been tempted to doubt whether any steam was actually generated.

Upon lifting up for an instant one fide of the bottle, and letting in a fmall quantity of cold air, the clouds instantly returned, and continued circulating feveral minutes with great rapidity, and then gradually difappeared as before. This Experiment was repeated feveral times, and always with the fame refult; the steam always becoming vifible when cold air was mixed with it, and afterwards recovering its transparency when, part of this air being expelled, that which remained had acquired the temperature of the steam.

Finding that cold air introduced under the bottle caufed the steam to be partially condensed, and clouds to be formed, I was defirous of feeing what vifible effects would be produced by introducing a cold folid body under the bottle. I imagined that if steam was a conductor of Heat, fome part of the Heat in the steam paffing out of it into the cold body, clouds would of courfe be formed; but I thought if steam was a *non-conductor* of Heat,—that is to fay, if *one particle of steam could not communicate any part of its Heat to its neighbouring particles*, in that cafe, as the cold body could only affect the particles of steam *actually in contact with it*, no cloud would appear; and the refult of the

Experiment showed that steam is in fact a *non-conductor of Heat*; for, notwithstanding the cold body used in this Experiment was very large and very cold, being a solid lump of ice nearly as large as an hen's egg, placed in the middle of the hollow cavity under the bottle, upon a small tripod or stand made of iron wire; yet as soon as the clouds which were formed in consequence of the unavoidable introduction of cold air in lifting up the bottle to introduce the ice, were dissipated, which soon happened, the steam became so perfectly transparent and invisible, that *not the smallest appearance of cloudiness was to be seen any where*, not even about the ice, which, as it went on to melt, appeared as clear and as transparent as a piece of the finest rock crystal.

This experiment, which I first made at Florence, in the month of November 1793, was repeated several times in the presence of Lord Palmerston, who was then at Florence, and Mons. de Fontana*.

In
 * The bottle made use of in this Experiment, though it appeared very large externally, contained but a very small quantity of water, owing to its bottom being very much drawn inwards. As the hollow cavity under the bottom of the bottle (which, as I just observed, was nearly in the form of a hemisphere, and 6 inches in diameter) served as a receiver for confining the steam, which rose from the boiling water in the plate, it may perhaps be imagined that a common glass receiver in the form of a bell, such as are used in Pneumatical Experiments, might answer as well as this bottle; I thought so myself, but upon making the experiment I found my mistake. A common receiver will answer perfectly well for confining the steam, but the glass soon becomes so hot that the drops of water which are formed upon its internal surface, in consequence of the condensation of the steam, instead of running down the sides of the receiver in clear
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In these Experiments the air was not entirely expelled from under the bottle; on the contrary, a considerable quantity of it remained mixed with the steam even after the clouds had totally disappeared, as I found by a particular Experiment made with a view to ascertain that fact; but that circumstance does not render the result of this Experiment less curious, on the contrary I think it tends to make it more surprising. It should seem that neither the mass of steam, nor that of air, were at all cooled by the body of ice which they surrounded, for if the air had been cooled, (in mass,) it seems highly probable that the clouds would have returned.

The results of these Experiments compared with those formerly alluded to, in which I had endeavoured to ascertain the most advantageous forms for boilers, opened to me an entirely new field for speculation and for improvement in the Management of Fire. They showed me that not only cold air, but also hot air, and hot steam, and hot mixtures of air and steam, are non-conductors of Heat; consequently that the hot vapour which

transparent streams, form blotches and streaks, which render the glass so opaque that nothing can be seen distinctly through it; and this of course completely frustrates the main design of the Experiment; but cold water in the bottle keeping the glass cool, the condensation of the steam upon the sides of the hollow cavity formed by the bottom of the bottle, goes on more regularly, and the streams of water which are continually running down the sides of the glass, uniting together, form one transparent sheet of water, by which means every thing that goes on under the bottle may be distinctly seen.

rises from burning Fuel, and even the *flame itself*, is a *non-conductor of Heat*.

This may be thought a bold assertion, but a little calm reflection, and a careful examination of the phenomena which attend the combustion of Fuel, and the communication of Heat by flame, will show it to be well founded; and the advantages which may be derived from the knowledge of this fact are of very great importance indeed. But this subject deserves to be thoroughly investigated.

C H A P. IV.

Of the MANNER in which HEAT is COMMUNICATED by FLAME to other Bodies.—Flame acts on Bodies in the same Manner as a hot Wind.—The Effect of a Blow-pipe in increasing the Activity of Flame explained, and illustrated by Experiments.—A Knowledge of the Manner in which Heat is communicated by Flame necessary in order to determine the most advantageous Forms for Boilers.—General Principles on which Boilers of all Dimensions ought be constructed.

IF FLAME be merely vapour, or a mixture of air and steam heated red-hot, as air and steam are both non-conductors of heat, there seems to be no difficulty in conceiving that Flame may, notwithstanding its great degree of heat, still retain the properties of its component fluids, and remain a *non-conductor of heat*. The non-conducting power of air does not appear to be at all impaired by being heated to the temperature of boiling water; and I see no reason why that property in air, or in any other elastic fluid, should be impaired by any augmentation of temperature however great. If steam, or if air, at the temperature of 212 degrees of Fahrenheit's thermometer, be a non-conductor

of heat, why should it not remain a non-conductor at that of 1000 degrees, or when heated red-hot? I confess I do not see how a body *could* be deprived of a property so essential, without being at the same time totally changed; and I believe nobody will imagine that either air or steam undergo any chymical change merely by being heated to the temperature of red-hot iron. But without insisting upon these reasonings, however conclusive I may think them, I shall endeavour to show, from experiment and observation, in short to *prove*, that Flame is in fact a non-conductor of heat.

Taking it for granted,—what I imagine will not be denied,—that air is a non-conductor of heat, at least in the sense I have used that appellation, I shall endeavour to show that Flame acts precisely in the same manner as a hot wind would do in communicating heat, and in no other way; and if I succeed in this, I fancy I may consider the proposition as sufficiently proved.

The effect of a blast of cold air in cooling any hot body exposed to it is well known, and the causes of this effect may easily be traced to that property of air which renders it a non-conductor of heat; for if the particles of cold air in contact with a hot body, could, with perfect facility, give the heat they acquire from the hot body to other particles of air by which they are immediately surrounded, and these again to others, and so on, the heat would be carried off *as fast as the hot body could part with it*, and any motion of the particles

of the air,—any wind, or blast, would not sensibly facilitate or hasten the cooling of the body; and by a parity of reasoning it may be shown, that if Flame were in fact a perfect conductor of heat, any cold body plunged into it would always be heated *as fast as that body could receive heat*; and neither any motion of the internal parts of the Flame, nor the velocity with which it impinged against the cold body, could have any sensible effect either to facilitate or accelerate the heating of the body. But if Flame be a non-conductor of heat, its action will be exactly similar to that of a hot wind, and consequently much will depend upon the manner in which it is applied to any body intended to be heated by it.—Those particles of it *only* which are in *actual contact* with the body will communicate heat to it; and the greater the number of different particles of the Flame which are brought into contact with it, the greater will be the quantity of heat communicated: Hence the importance of causing the Flame to impinge with force against the body to be heated, and to strike it in such a manner that its current may be broken, and that whirlpools may be formed in it; for the rapid motion of the Flame causes a quick succession of hot particles; and admitting our assumed principles to be true, it is quite evident that every kind of internal motion among the particles of the Flame by which it can be agitated, must tend very powerfully to accelerate the communication of the heat.

The

The effect of a blow-pipe is well known, but I do not think that the *manner* in which it increases the *action* of *Flame*, has ever been satisfactorily explained. It has generally been imagined, I believe, that the current of fresh air which is forced through the *Flame* by a blow-pipe actually increases the quantity of heat; I rather suppose it does little more than direct the heat *actually existing in the Flame* to a given point. A current of air cannot generate heat, without at the same time being decomposed; and in order to its being decomposed in a fire, it must be brought into actual contact with the burning Fuel, or at least with the uninfamed inflammable vapour which rises from it:—But can it be supposed that there can be any thing inflammable, and not actually inflamed, in the clear, bright, and perfectly transparent *Flame* of a wax candle?—A blow-pipe has however as sensible an effect, when directed against the clear *Flame* of a wax candle, as when it is employed to increase the action of a common glass-worker's lamp.

Conceiving that the discovery of the *manner* in which the current of air from a blow-pipe serves to increase the intensity of the action of the *Flame*, could not fail to throw much light upon the subject under consideration,—namely, the investigation of *the manner* in which heat is communicated to bodies by *Flame*,—I made the following Experiments, the results of which I conceive to be decisive.

Concluding that the current of air from a blow-pipe, directed against the Flame of any burning body, could tend to increase the intensity of the action of the Flame only in one or both of these two ways, namely, by increasing its *action* upon the body against which it is directed; or by actually increasing the *quantity* of heat generated in the combustion of the Fuel; a method occurred to me by which I thought it possible to determine, by actual experiment, to which of these causes the effect in question is owing, or how much each of them might contribute to it. To do this I filled a large bladder, containing above a gallon, with *fixed air*, which, as is well known, is totally unfit for supporting the combustion of inflammable bodies, and which, of course, could not be suspected of *adding* any heat to a Flame against which a current of it should be directed; I imagined therefore that if a blow-pipe supplied with this air, on being directed against the Flame of a candle, should be found to produce nearly the same effect as when common air is used for the same purpose, it would prove to a demonstration that the augmentation of the intensity of the action, or activity of the Flame which arises from the use of a blow-pipe, is owing to the agitation of the Flame,—to its being directed to a point,—to the impetuosity with which it is made to strike against the body which is heated by it,—and to the rapid succession of fresh particles of this hot vapour, and not to any *positive increase of heat*.

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A blow-pipe being attached to the bladder containing fixed air, the end of this pipe was directed to the clear brilliant flame of a wax candle, which had just been snuffed; and, by compressing the bladder, the Flame was projected against a small tube of glass, which was very soon made red-hot, and even melted.

Having repeated this experiment several times, and having found how long it required to melt the tube when the Flame of the candle was forced against it by a blast of *fixed air*, I now varied the experiment, by making use of common atmospheric air, instead of fixed air; taking care to employ the same candle and the same blow-pipe used in the former experiments, and even making use of the bladder, in order that the experiments being exactly similar, and differing only in the kinds of air made use of, the effect of that difference might be discovered and estimated.

The results of these experiments were most perfectly conclusive; and proved in a decisive manner, that the effect of a blow-pipe, *when applied to clear Flame*, arises not from any real augmentation of heat, but merely from the increased activity of the Flame, in consequence of its being impelled with force, and broken in eddies on the surface of the body against which it is made to act; the effect of the blow-pipe in these experiments being to all appearance quite as great when fixed air was made use of, (which *could not* increase the quantity of heat,) as when atmospheric air was used.

But conceiving the determination of this question relative to the manner in which Flame communicates heat, to be a matter of much importance, I did not rest my inquiries here: I repeated the experiments very often, and varied them in a great number of different ways; sometimes making use of fixed air; sometimes of atmospheric air; and at other times using dephlogisticated air; and common air rendered unfit for the support of animal life and of combustion, by burning a candle in it till the candle went out.

It would take up too much time to give an account in detail of all these experiments; I shall therefore content myself with merely observing, that they all tended to show that the effect of a blow-pipe *used in the manner here described*, is owing to the direction and velocity it gives to the Flame against which it is employed, and not to any real increase of heat.

It must be remembered that the principal object I had in view in these experiments was to discover the *manner* in which Flame communicates heat to other bodies, and by what means that communication may be facilitated.—Were it required to increase the intensity of the heat by *blowing the fire*, the current of air must be applied in such a manner as to expedite the combustion; it must be directed to the inflamed surface of the burning Fuel, and not to the red-hot vapour or flame which rises from it, and in which the combustion is most probably already quite complete; and in this case there is

no doubt but the effect produced by blowing would depend much upon the quality of the air made use of.

The results of the foregoing experiments with the blow-pipe will, I am confident, be thought quite conclusive by those who will take the trouble to consider them attentively,—and the advantages that may be derived from the knowledge of the fact established by them are very obvious. If Flame, or the hot vapour which rises from burning bodies, be a non-conductor of heat;—and if, in order to communicate its heat to any other body, it be necessary that its particles *individually* be brought into actual contact with that body; it is evident that the form of a boiler, and of its fire-place, must be matters of much importance; and that *that form* must be most advantageous, which is best calculated to produce an internal motion in the Flame, and to bring alternately as many of its particles as possible into contact with the body which is to be heated by it. The boiler must not only have as large a surface as possible, but it must be of such a form as to cause the Flame which embraces it—to impinge against it with force—to break against it—and to play over its surface in eddies and whirlpools.

It is therefore against the *bottom* of a boiler, and not against its sides, that the principal efforts of the Flame must be directed; for when the Flame, or hot vapour, is permitted to rise freely by the vertical sides of a boiler, it slides over its surface very rapidly, and there being no obstacle in the way to
break

break the Flame into eddies and whirlpools, it glides quietly on like a stream of water in a smooth canal; and the same hot particles of this vapour which happen to be in immediate contact with the sides of the boiler at its bottom or lower extremity, being continually pressed against the surface of the boiler as they are forced upwards by the rising current, prevent other hot particles from approaching the boiler; so that by far the greatest part of the heat in the Flame and hot vapour which rise from the Fire, instead of entering the boiler, goes off into the atmosphere by the chimney, and is totally lost.

The amount of this loss of heat, arising from the faulty construction of boilers and their fire-places, may be estimated from the results of the Experiments recorded in the following Chapter.

CHAP. V.

An Account of Experiments made with Boilers and Fire-places of various Forms and Dimensions; together with Remarks and Observations on their Results, and on the Improvements that may be derived from them.—An Account of some Experiments made on a very large Scale in a Brew-house Boiler.—An Account of a Brew-house Boiler constructed and fitted up on an improved Plan.—Results of several Experiments which were made with this new Boiler.—Of the Advantage in regard to the Economy of Fuel in boiling Liquids, which arises from performing that Process on a large Scale.—These Advantages are limited.—An Account of an Alteration which was made in the new Brew-house Boiler, with a view to the SAVING OF TIME in causing its Contents to boil.—Experiments showing the Effects produced by these Alterations.—An Estimate of the RELATIVE QUANTITIES OF HEAT producible from COAKS—PIT-COAL—CHARCOAL, and OAK.—A Method of estimating the Quantity of Pit-coal which would be necessary to perform any of the Processes mentioned in this Essay, in which Wood was used as Fuel.—An Estimate of the TOTAL QUANTITIES of Heat producible in the Combustion of different Kinds of Fuel; and of the real Quantities of Heat

Heat which are lost, under various Circumstances, in culinary Processes.

WHAT has been said in the foregoing Chapter will, I trust, be sufficient to give my reader a clear and distinct idea of the subject under consideration, in all its various details and connections,—and enable him to comprehend, without the smallest difficulty, every thing I have to add on this subject; and particularly to discover the different objects I had in view in the Experiments of which I am now about to give an account, and to judge with facility and certainty of the conclusions I have drawn from their results.

These Experiments, though they occupy so many pages in this Essay, are but a small part of those I have made, and caused to be made under my direction, on the subject of Heat, during the last seven years. Were I to publish them all, with all their details as they are recorded in the register that has been kept of them, they would fill several volumes.

It was most fortunate for me that this register is very voluminous; for had it not been so, I should in all probability have taken it with me to England last year, and in that case I should have lost it, with the rest of my papers, in the trunk of which I was robbed in passing through St. Paul's Church-yard, on my arrival in London after an absence of eleven years*.

* I have many reasons to think that these papers are still in being;—what an everlasting obligation should I be under to the person who would cause them to be returned to me!

As I foresaw, when I first began my inquiries respecting Heat, that I should have occasion to make many Experiments on boiling Liquids, to facilitate the registering of them I formed a Table, (which I had printed,) in which, under various heads, every circumstance relative to any common Experiment of the kind in question could be entered with much regularity, and with little trouble.

As this Table may be useful to others who may be engaged in similar pursuits, and as the publishing of it will also tend to give my reader a more perfect idea of the manner in which my Experiments were conducted, I shall (as an example) give an account of one Experiment, *in the same form* in which it was registered in one of these printed Tables.

These Tables, as they are printed for use, (on detached sheets,) occupy one side of half a sheet of common folio writing-paper.

	common folio writing-paper.	
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AN EXPERIMENT ON THE MANAGEMENT OF FIRE IN BOILING LIQUIDS;
Made at [MUNICH], in [a Brew-houfe belonging to the Elector], the [15th] of [April 1795].

Time of the Day.		Fuel put into the Fire-place.		Temperature of the Liquid.		Contents of the Boiler.			Height of the Barometer, 26 $\frac{5}{12}$ Inches; of Thermometer, 580.
Hour.	Minutes.	Numb. of Pieces.	In Weight.			Kind of Liquid, &c.	Measures.	In Weight.	
5	15	29	lbs. 100	60°		[Water.]	6984	lbs. 12508	DIMENSIONS OF THE BOILER. Diameter { above — of long — 10 Feet. below — and wide — 8 Feet. Depth 4 Feet. — Was constructed of [Copper], and weighed [not known]; contained of Water 8176 Measures, weighing 14843 lbs. KIND OF FUEL USED. — [Pine-wood, Moderately dry, in Lengths of 6 Feet.] GENERAL RESULTS OF THE EXPERIMENT. Time employed to make the Liquid boil, — [3.40] min. Fuel consumed to make the Liquid boil, — [840] lbs. Time the Liquid continued to boil, — [1.43] min. Fuel added to keep the Liquid boiling. — 100 lbs. Quantity of the Liquid evaporated [not observed]. GENERAL RESULT. With the Heat generated in the Combustion of 1 lb. of the Fuel, [19.2 lbs.] of ice-cold Water, raised to boil, or [139.80 lbs.] of boiling-hot Water kept boiling 1 Hour.
—	30	6	50	70°					
—	41	6	50	—					
10	5	7	50	92°					
—	25	7	50	105°					
—	46	7	50	120°					
11	0	7	50	130°					
—	15	7	50	145°					
—	26	8	50	155°					
—	43	7	50	163°					
12	50	7	50	173°					
—	5	8	50	183°					
—	17	7	50	192°					
—	31	15	100	[boiled].					
—	55	—	—	—					
1	38	7	50	—					
2	35	7	50	—					
3	38	—	—	[ceased boiling].					

Every thing in this Table, except such figures and words as are printed between crochets, is contained in the printed forms: Hence it is evident how much these Tables tend to diminish the trouble of registering the results of Experiments of this kind, and also to prevent mistakes.

The example I have here given is an account of an Experiment, in which a very large quantity of water, equal to 15,590 lbs. Avoirdupois in weight, or 1866 wine gallons of 231 cubic inches each; but it is evident that these Tables answer equally well for the small quantity contained by the smallest saucepan.

The height of the barometer is expressed in Paris inches; that of the thermometer in degrees of Fahrenheit's scale.—The other measures, as well of length as of capacity, are the common measures of the country (Bavaria); and the weight is expressed in Bavarian pounds, of which 100 make 123.84 lbs. Avoirdupois.

What is entered under the head of GENERAL RESULTS OF THE EXPERIMENT, requires no explanation; but what I have called the PRECISE RESULT must be explained.

Having frequent occasion to compare the results of Experiments made at different times and in different seasons of the year, as the temperature of the water in the Boiler when the fire is lighted under it is seldom the same in any two Experiments, and as the boiling heat varies with the variations of the pressure of the atmosphere, or of the height of the mercury

mercury in the barometer, it became necessary to make proper allowances for these differences. This I thought could best be done by determining, by computation, from the number of degrees the water was *actually heated*, and the quantity of Fuel consumed in heating it that number of degrees, how much Fuel would have been required to have it heated 180 degrees, or from the point of freezing to that of boiling water (the boiling point being taken equal to the temperature indicated by 212° of Fahrenheit's thermometer, which is the boiling point under the mean pressure of the atmosphere at the surface of the sea): Then, by dividing the weight of the water used in the Experiment, (expressed in pounds,) by the weight of the Fuel expressed in pounds necessary to heat it 180 degrees, or from the temperature of freezing to that of boiling water; this gives the number of pounds of ice-cold water which (according to the result of the given Experiment) *might have been* made to boil, — with the heat generated in the combustion of 1 lb. of the Fuel, under the mean pressure of the atmosphere at the level of the surface of the sea.

The city of Munich, where all the Experiments were made of which I am about to give an account, being situated almost in the centre of Germany, lies very high above the level of the sea. The mean height of the mercury in the barometer is only about 28 English inches, consequently water boils at Munich at a lower temperature than at London. The difference is even too considerable

to be neglected, it amounts to $2\frac{1}{2}$ degrees of Fahrenheit's scale,—being $209\frac{1}{2}$ degrees at a medium at Munich, and 212 degrees in all places situated near the level of the sea. To render the results of my experiments and computations more simple and more generally useful, I shall always make due allowance for this difference.

Having, from the actual result of each Experiment, made a computation on the principles here described, showing what (for the want of a better expression) I have called the *Precise Result* of the Experiment, it is evident that these computations show very accurately the comparative merit of the mechanical arrangements, and the management of the Fire in conducting the Experiments, in as far as relates to the Economy of Fuel; for the more ice-cold water that can be made to boil with the heat generated in the combustion of any given quantity (1 lb. for instance) of Fuel, the more perfect of course (other things being equal) must be the construction of the fire-place.

Under the head of *PRECISE RESULT* I have sometimes added another computation, showing how much "*boiling-hot water*" might, according to the result of the given Experiment, be *kept boiling "one hour"* with the heat generated in the combustion of "*1 lb. of the Fuel.*" Though I have called this a *Precise Result*, it is evident that in most cases it cannot be considered as being very exact, owing to the difficulty of estimating the quantity of Fuel in the fire-place, which is *unconsumed at the moment when the water begins to boil.*

In the foregoing example, in making this computation I supposed that, when the water began to boil, there was wood enough in the fire-place *unconsumed* to keep the water boiling 43 minutes, and that the wood added afterwards (100 lb.) kept the water boiling the remainder of the time it boiled, or just 2 hours.

In most cases, however, to save trouble in making these computations, I have supposed that all the wood employed in making the water boil is entirely consumed in that process, and that all the heat expended in *keeping the water boiling* is furnished by the Fuel which is added *after the water had begun to boil*. This supposition is evidently erroneous; but as the computation in question can at best give but an inaccurate and doubtful result, labour bestowed on it would be thrown away: But imperfect as these rough estimates are, they will however in many cases be found useful.

In giving an account of the following Experiments, I shall not place them exactly in the order in which they were made, but shall arrange them in such a manner as I shall think best, in order that the information derived from their results may appear in a clear point of view.

For greater convenience in referring to them, I shall number them all; and as I have already given numbers to the four I mentioned in the First Chapter of this Essay, I shall proceed in regular order with the rest.

Experi-

Experiment, No. 5.

The first kitchen of the House of Industry at Munich has already been described in the First Chapter of this Essay; and it was there mentioned, that the daily expence of Fuel in that kitchen, when food (peas-soup) was prepared for 1000 persons, amounted to 300 lb. in weight of dry beech-wood. Now as each portion of soup consisted of 1 lb., this gives 0.3 of a pound of wood for each pound of soup.

Experiment, No. 6.

The first kitchen of the House of Industry having been pulled down, it was afterwards rebuilt on a different principle. Instead of Copper Boilers, Iron Boilers of a hemispherical form were now used, and each of these Boilers had its own separate closed fire-place. The Boiler being suspended by its rim in the brick-work, and room being left for the flame to play all round it. The smoke went off into the chimney by an horizontal canal, 5 inches wide and 5 inches high, which was concealed in the mass of brick-work, and which opened into the fire-place on the side opposite to the opening by which the Fuel was introduced.

The Fire was made on a flat iron grate placed directly under the Boiler, and distant from its bottom about 12 inches. The ash-pit door was fur-

nished with a register ; but there was no damper to the canal by which the smoke went off into the chimney, which was a very great defect. The opening into the fire-place was closed by an iron door. Each of these Iron Boilers weighed about 148 lbs. Avoirdupois, was $25\frac{1}{4}$ English inches in diameter, and 14.935 inches deep, and contained $190\frac{1}{2}$ lbs. Bavarian weight of water, equal to 235.91 lbs. Avoirdupois, or about $28\frac{1}{4}$ English wine-gallons.

From this account of the manner in which these Iron Boilers were fitted up, it is evident that the arrangement was not essentially different from that of kitchens for hospitals, as they are commonly constructed.

From Experiments made with care, and often repeated, I found that to prepare 89 portions (or 89 lbs. Bavarian weight) of peas-soup in one of these Boilers, 43 lbs. of *dry beech-wood* were required as Fuel, and that the process lasted four hours and an half: This gives 0.483 of a pound of wood for each pound of the soup.

In the first arrangement of this kitchen, only 0.3 of a pound of wood was required to prepare 1 lb. of soup: Hence it appears that the kitchen had not been improved,—considered with a view to the Economy of Fuel,—by the alterations which had been made in it. This was what I expected; for the object I had in view in constructing this kitchen was not to save Fuel, but to find out how much of it is wasted in culinary processes, as they

they are commonly performed on a large scale in hospitals and other institutions of public charity.— Till I knew this, it was not in my power to estimate, with any degree of precision, the advantages of any improvements I might introduce in the construction of kitchen fire-places.

To determine in how far the quantity of Fuel necessary in any given culinary process depends on the form of *the fire-place*, (the Boiler and every other circumstance remaining the same,) I made the following Experiments.

Experiments, No. 7 and No. 8.

Two of the Iron Boilers in the kitchen of the House of Industry (which, as they were both cast from the same model, were as near alike as possible) being chosen for this Experiment, one of them (No. 8.) being taken out of the brick-work, its fire-place was altered and fitted up anew on improved principles. The grate was made circular and concave, and its diameter was reduced to 12 inches; the fire-place was made cylindrical above the grate, and only 12 inches in diameter; and the Boiler being seated on the top of the wall of this cylindrical fire-place, the flame passing through a small opening on one side of the fire-place, at the top of it, made one complete turn about the Boiler before it was permitted to go off into the canal by which the smoke passed off into the chimney.

Though there was no damper in this canal, yet as its entrance or opening, where it joined the canal which went round the Boiler, was considerably reduced in size, this answered (though imperfectly) the purpose of a damper. This fire-place being completed, and a small Fire having been kept up in it for several days to dry the masonry, the Experiment was made by preparing the same quantity of the same kind of soup in this, and in a neighbouring Boiler whose fire-place had not been altered.

The food cooked in each was 89 lb. of Peas-soup; and the Experiment was begun and finished in both Boilers at the same time.

The wood employed as Fuel was pine; and it had been thoroughly dried in an oven the day before it was used.

The Boilers were both kept constantly covered with their double covers, except only when the Soup was stirred about to prevent its burning to the bottoms of the Boilers.

The result of this interesting Experiment was as follows:

	Experiment No. 7.	Experiment No. 8.
	In the Boiler No. 1.	In the Boiler No. 2, with the im- proved Fire- Place.
Quantity of wood consumed in cook- ing 89 lbs. Bavarian weight of Peas-soup - - - - -	37 lbs.	14 lbs.

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These Experiments were made on the 7th of November 1794. On repeating them the next day with pine-wood, which had not been previously dried in an oven, the result was as follows :

Experiments, No. 9 and No. 10.

	Experiment No. 9.	Experiment No. 10.
	In the Boiler No. 1.	In the Boiler No. 8, with the im- proved Fire- place.
Quantity of wood consumed in cook- ing 89 lbs. of Peas-soup	39 lbs.	16 lbs.

The first remark I shall make on the results of these Experiments is the proof they afford, by comparing them with that which preceded them (No. 6.), of the important fact, that pine-wood affords more heat in its combustion than beech. This fact is the more extraordinary, as it is directly contrary to the opinion generally entertained on that subject; and it is the more important, as the price of pine-wood is, in most places, only about half as high as that of beech, when the quantities, *estimated by weight*, are equal.

In the Experiment No. 6. it was found, that 43 lb. of dry beech-wood were necessary, when used as Fuel, to prepare 89 lbs. of Peas-soup. In the Experiment No. 7. the same process was performed with 37 lb. and in the Experiment No. 9.

with 39 lb. of dry pine. But I shall have occasion to treat this subject more at length in another place. In the mean time I would, however, just observe, that all my Experiments have uniformly tended to confirm the fact, that dry pine-wood affords more heat in combustion than dry beech. I have reason to think the difference is in fact greater than the Experiments before us indicate; but the *apparent* amount of it will always depend in a great measure on the circumstances under which the Fuel is consumed; or, in other words, on the construction of the fire-place; and it is no small advantage attending the fire-places I shall recommend, that they are so contrived as to increase, as much as it is possible, the superiority of the most common and cheapest fire-wood over that which is more scarce and costly.

By comparing the results of these two sets of Experiments (No. 7 and No. 8, No. 9 and No. 10.) an estimate may be made of the advantage of using *very dry wood* for Fuel, instead of making use of wood that has been less thoroughly dried; but as I mean to take an opportunity of investigating that matter also more carefully hereafter, I shall not at present enlarge on it farther than just to observe, that as the wood, which was dried in an oven, was weighed for use after it had been dried, and as it certainly weighed more before it was put into the oven, the real saving arising from using it in this dried state is not so great as the difference in the weights of the quantities of wood used in the two
Expe-

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Experiments. To estimate that saving with precision, the wood should be weighed before it is dried, or in the same state in which the other parcel of wood, which is used without being dried, is weighed.

But to proceed to the principal object I had in view in these Experiments;—the determination of the effects of the difference in the construction of the two fire-places;—the difference in the quantity of Fuel expended in the two fire-places in performing the same process, shows, in a manner which does not stand in need of any illustration, how much had been gained by the improvements which had been introduced.

Conceiving it to be an object of great importance to ascertain by actual experiment, and with as much precision as possible, the real amount of the advantages, in regard to the Economy of Fuel, that may be derived from improvements in the forms of fire-places, I did not content myself with improving from time to time the kitchens I had constructed, but I took pains to determine how much I had gained by each alteration that was made. This was necessary, not only to furnish myself with more forcible arguments to induce others to adopt my improvements, but also to satisfy myself with regard to the progress I made in my investigations.

In the first arrangement of the kitchen of the Military Academy, the Boilers were suspended by their rims in the brick-work in such a manner that
the

the flame could pass freely all round them, and the smoke went off in horizontal canals which led to the chimney, but which were not furnished with dampers.

The Fire was made on a flat square iron-grate; and the internal diameter of the fire-place was 2 or 3 inches larger than the diameter of the Boiler which belonged to it. The bottom of the Boiler was from 6 to 10, or 12 inches (according to its size) above the level of the grate; and the door of the opening into the fire-place, by which the Fuel was introduced, was kept constantly closed. The ash-pit door was furnished with a register, and the Boilers were all furnished with double covers.

Having, in consequence of the progress I had made in my inquiries respecting the Management of Heat, and the Economy of Fuel, come to a resolution to pull down this kitchen, and rebuild it on an improved principle; previous to its being demolished, I made several very accurate Experiments to determine the real expence of Fuel in the fire-places as they *then existed*, with all their faults; and when the new arrangement of the kitchen was completed, I repeated these Experiments *with the same Boilers*; and by comparing the results of these two sets of Experiments, I was able to estimate with great precision the real amount of the saving of time as well as of Fuel,—which were derived from the improvements I had introduced.

After all that has been said (and perhaps already too often repeated in different parts of this Essay)

on the construction of fire-places, my reader will be able to form a clear and just idea of the construction of those of which I am now speaking, (those of the kitchen of the Military Academy, in its *present* improved state,) when he is told that the Fire burns on a circular concave iron grate, about half the diameter of the circular Boiler which belongs to the fire-place; that the fire-place, properly so called, is a cylindrical cavity in the solid brick-work which supports the Boiler, equal in diameter to the circular grate, and from six to ten inches high, more or less according to the size of the Boiler; that the Boiler is *set down* on the top of the circular wall which forms this fire-place, a small opening, from three to four or five inches in length taken horizontally, and about two or three inches high, being left on one side of this wall at the top of it, that the flame which burns up under the middle of the bottom of the Boiler may afterwards pass round (in a spiral canal constructed for that purpose) under that part of the bottom of the Boiler which lies *without* the top of the wall of the fire-place on which the Boiler reposes. The flame having made one complete turn *under* the Boiler in this spiral canal, it rises upwards, and going once *round the sides of the Boiler*, goes off by an horizontal canal, furnished with a damper, into the chimney.

In order that the top of the circular wall of the fire-place on which the Boiler is seated, may not cover

cover too much of the bottom of the Boiler, its thickness is suddenly reduced *in that part* (that is to say, just where it touches the Boiler) to about half an inch.

The opening by which the Fuel is introduced into the fire-place, is a conical hole in a piece of fire-stone, which hole is closed by a fit stopper made of the same kind of stone. The ash-pit door and its register are finished with so much nicety, that when they are quite closed the Fire almost instantaneously goes out.

The dimensions of the Boiler, in which the Experiments of which I am about to give an account were made, are as follows :

Diameter	{ above - 14.935 }	Inches, English measure.
	{ below - 13.39 }	
Depth	- 14.52	

It weighs 37 lbs. Avoirdupois; and it contains, when quite full, about 73 lbs. Avoirdupois, equal to $8\frac{1}{2}$ gallons (wine-measure) of water.

In two Experiments with this Boiler, which were both made by myself, and in which attention was paid to every circumstance that could tend to render them perfect, the results were as follows :

	Experiment No. 11.	Experiment No. 12.
	The first Fire-place.	The improved Fire-place.
Quantity of water in the Boiler, in <i>Bavarian pounds</i> - - -	43.63 lbs.	43.63 lbs.
Temperature of the water in the Boiler at the beginning of the Experiment - - -	59°	60°
Time employed in making the water boil - - -	67 min.	30 min.
Wood consumed in making the water boil, in <i>Bavarian pounds</i>	9 lbs.	3 lbs.
Time the water continued boil- ing - - -	2 hrs. 2 min.	3 hours
Wood added to keep the water boiling - - -	5 lbs.	2½ lbs.
Kind of wood used - - -	Pine	Pine

Precise Results.

Ice-cold water heated 180 de- grees, or made to boil, with 1 lb. of wood - - -	4.02 lbs.	11.93 lbs.
Boiling-hot water kept boiling 1 hour, with 1 lb. of wood - -	17.74 lbs.	52.36 lbs.

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The following Experiments were made with two Copper Boilers, (No. 1 and No. 2.) nearly of the same dimensions, in the kitchen of the Military Academy at Munich, in the present improved state of that kitchen. These Boilers are round and deep, and weigh each about 62 lbs. Avoirdupois. They belonged originally to the kitchen of the House of Industry, being two of the eight Boilers which, in the first arrangement of that kitchen, were heated by the same Fire.

Their exact dimensions, measured in English inches, are as follows :

			The Boiler No. 1.	The Boiler No. 2.
			Inches.	Inches.
Diameter	above	—	22.66	22.66
	below	—	19.82	20.85
Depth	—	—	24.72	22.04

At the beginning of each of the following Experiments, each of these Boilers contained just 95 measures (or *Bavarian maasse*) of water, weighing 187 lbs. Bavarian weight, (equal to 232.58 lbs. Avoirdupois,) or a trifle less than 28 gallons.

The grate on which the Fire was made under each of these Boilers is circular and concave, and 11 inches in diameter; and their fire-places are in all respects similar to that just described (Experiment No. 11.). Both Boilers are furnished with double covers.

The

The Experiments made with the Boiler No. 1. and their results, were as follows :

	Experiment No. 13.	Experiment No. 14.	Experiment No. 15.	Experiment No. 16.
Quantity of water in the Boiler in the beginning of the Experiment - - -	lbs. 187	lbs. 187	lbs. 187	lbs. 187
Temperature of the water in the Boiler at the beginning of the Experiment -	61°	59°	64°	55½°
Time employed in making the water boil - - - - -	min. 78	min. 61	min. 61	min. 62
Wood consumed in making the water boil - - - - -	lbs. 12	lbs. 11	lbs. 9	lbs. 8
Time the water continued to boil - - -	min. 17	min. 28	min. 6	h. min. 2 19
Quantity of Fuel added to keep it boiling this time - -	—	—	—	lbs. 4
Kind of wood used as Fuel - - - - -	Beech	Beech	Pine	Pine

Precise Results of the Experiments.

Ice-cold water heated 180°, or made to boil, with the heat generated in the combustion of 1 lb. of the Fuel - -	lbs. 12.89	lbs. 14.15	lbs. 16.89	lbs. 20
Boiling water kept boiling one hour, with the heat generated in the combustion of 1 lb. of the wood - - - - -	—	—	—	lbs. 108.40

All

All the foregoing Experiments were made on the same day, (the 13th of October 1794,) and in the same order in which they are numbered.

The following are the results of the Experiments made with the Boiler No. 2.

	Experm. No. 17.	Experm. No. 18.	Experm. No. 19.	Experm. No. 20.	Experm. No. 21.
Quantity of water in the Boiler at the beginning of the Experiment, in <i>Bavarian pounds</i> —	lbs. 187	lbs. 187	lbs. 187	lbs. 187	lbs. 187
Temperature of the water in the Boiler at the beginning of the Experiment —	61°	58°	60°	55°	212°
Time employed in making the water boil —	min. 75	min. 55	min. 57	min. 60	—
Wood consumed in making the water boil —	lbs. 11	lbs. 11	lbs. 9	lbs. 8	—
Time the water continued to boil —	min. 21	min. 17	min. 8	h. min. 2 29	h. min. 1 10
Wood added to keep the water boiling —	lb. 1	—	—	lbs. 3½	lbs. 1½
Kind of wood used —	Beech	Beech	Pine	Pine	Beech
<i>Precise Results.</i>					
Ice-cold water heated 180°, or made to boil, with 1 lb. of wood —	lbs. 13.92	lbs. 14.33	lbs. 17.59	lbs. 20.10	—
Boiling-hot water kept boiling one hour with 1 lb. of wood —	—	—	—	lbs. 132.68	lbs. 145.44

This set of Experiments was made at the same time with the foregoing set, namely, on the 13th October 1794, and they were made in the order in which they are here registered. In the last but one, (No. 20.) the Economy of Fuel in the process of heating water was carried farther than in any other Experiment I have ever made.

In the following Experiments, which were made in a large Copper Boiler fitted up on my most improved principles, belonging to the kitchen of the House of Industry, the Economy of Fuel was carried nearly as far.

This Boiler, which is circular, is $42\frac{1}{2}$ English inches in diameter above; 42.17 inches in diameter below; and 18.54 inches deep. It weighs $78\frac{1}{2}$ lbs. Avoirdupois; and contains, when quite full, 714 lbs. *Bavarian weight* (= 884 lbs. Avoirdupois, or 106 gallons) of water, at the temperature of 55° .

It is surrounded above by a wooden ring about two inches in thickness, into which it is fitted; and in this ring, in a groove about $\frac{1}{4}$ of an inch deep, is fitted a circular wooden flat cover; this cover is formed in three pieces, united by iron hinges; and one of these pieces being fastened down by hooks to the Boiler, the other two are so contrived as to be folded back upon it occasionally. From the upper surface of the part of the cover which is fastened down on the Boiler, a tin tube two inches in diameter, furnished with a damper, is fixed, by which the steam is carried off into a narrow wooden tube, which conducts it through an opening in the roof of the house into the open air.

To prevent still more effectually the escape of the heat through the wooden cover of the Boiler, the upper surface of it is protected from the cold atmosphere by a thick circular blanket covered on both sides by strong canvas, which is occasionally thrown over it.

Though the diameter of this Boiler below is more than 40 inches, the diameter of its fire-place (which is just under its center) is only 11 inches; but as the flame makes two complete turns under the bottom of the Boiler in a spiral canal, and one turn round it, the time required to heat it is not so great as, from the smallness of its fire-place, might have been expected.

It has ever been, and still continues to be, the decided favourite of the cook-maids.

The wood used as Fuel in the following Experiment was pine moderately dried. The billets were six inches long, and from one to two inches in diameter.

The following Table shows the results of five Experiments that were made with this Boiler by myself, just after it was fitted up:

	Experi- No. 22.	Experi- No. 23.	Experi- No. 24.	Experi- No. 25.	Experi- No. 26.
Quantity of water in the Boiler, in Bavarian pounds — — —	lbs. 508	lbs. 127	lbs. 254	lbs. 508	lbs. 508
Temperature of the wa- ter at the beginning of the Experiment —	48°	48°	96°	48°	48°
Time required to make the water boil — —	h. min. 2 4	min. 51	h. min. 1 15	h. min. 2 35	h. min. 3 1
Fuel employ'd to make the water boil — —	lbs. 24 $\frac{1}{2}$	lbs. 8 $\frac{1}{2}$	lbs. 12 $\frac{1}{2}$	lbs. 25	lbs. 24
Time the water conti- nued boiling — —	h. 3	—	—	h. 3	—
Fuel added to keep the water boiling — —	lbs. 6 $\frac{1}{2}$	—	—	lbs. 4 $\frac{1}{2}$	—
PRECISE RESULTS of the Experiments with the heat generated in the combustion of 175. of the Fuel.					
Ice-cold water heated 180°, or made to boil	lbs. 18.87	lbs. 12.74	lbs. 12.69	lbs. 17.48	lbs. 19.01
Or boiling-hot water kept boiling one hour	236.61	—	—	338.66	—

Without

Without stopping to make any observations on the results of these Experiments, (though they afford matter for several of an interesting nature,) I shall proceed to give a brief account of another set of Experiments, on a much larger scale, which were made in the Copper Boiler of a Brewery belonging to the Elector.

This Boiler, which is rectangular, is ten feet long, eight feet wide, and four feet deep, *Bavarian measure**, and contains 8176 *Bavarian maasse*, or measures, equal to 1866 gallons wine measure. On examining this Boiler, I found its fire-place was constructed on very bad principles; and on inquiry respecting the quantity of fire-wood consumed in it, I found the waste of Fuel to be very great.

This Brewery is used for making small *white* beer, (as from its pale colour it is called,) from malt made of wheat; and as it is worked all the year round, the expence of Fuel was very great, and the economy of it an object of considerable importance.

The quantity of fire-wood (pine) that had at an average been consumed daily in this Brewery was rather more than four *Bavarian clafters*, or cords. On altering the fire-place of this Brewery, and putting a (wooden) cover to the Boiler, I reduced this expence to less than $1\frac{1}{2}$ *clafters*.

In the new fire-place which I caused to be constructed for this Boiler, the cavity under the Boiler

* 100 *Bavarian inches* are equal to $95\frac{1}{2}$ *inches English measure*.

is divided into three flues, by thin brick walls which run in the direction of the length of the Boiler. The middle flue, which is twice as wide as one of the side flues, is occupied by the burning Fuel, and is furnished with a grate 20 inches wide, and 36 inches long, and the opening by which the Fuel is introduced into the fire-place is closed by two iron doors, placed one behind the other, at the distance of eight inches. The grate, which is placed at the hither end of the fire-place, is horized, and it is situated about twenty inches below the bottom of the Boiler. The air which serves to feed the Fire, is let in under the grate through a register in the ash-pit door.

When the double doors which close the entrance into the fire-place are shut, the flame of the burning Fuel first rises perpendicularly against the bottom of the Boiler; it then passes along to the farther end of the (middle) flue, which constitutes the fire-place, where it separates, and returns in the two side flues; it then rises up into two horizontal flues (one situated over the other) which go all round the Boiler; and having made the circuit of the Boiler, it goes off into separate canals (furnished with dampers) into the chimney.

Though the Figures 17 and 18, Plate III. are not drawings from the fire-place I am now describing, but of another which I shall soon have occasion to describe, yet an inspection of these figures will be found useful in forming an idea of the principles on which the fire-place in question was constructed, and

and on that account I shall occasionally refer to them.

The burning Fuel being confined within a narrow compass,—being well supplied with fresh air,—and being surrounded on all sides by thin walls of brick, (which are non-conductors,) the heat of the fire is most intense, and the combustion of the Fuel of course very complete. The flame, which is clear and vivid in the highest degree, and perfectly unmingled with smoke, runs rapidly along the bottom of the Boiler, (which forms the top of the flues,) and from the resistance it meets with in its passage, from friction, and from the number of turns it is obliged to make, it is thrown into innumerable eddies and whirlpools, and really affords a most entertaining spectacle.

That I might be able to enjoy at my ease this amusing sight, I caused a glass window to be made in the front wall of the fire-place, through which I could look into the Fire when the fire-place doors were shut; and I was well paid for the trouble and the trifling expence I had in getting it executed.

Some may be tempted to smile at what they may think a childish invention; but there are many others, I am confident, and among these many grave philosophers, who would have been very glad to have shared my amusement.

The window of which I am speaking is circular, and only six inches in diameter; but as the hole in the wall is conical, and much larger within than without, the field of this window (if I may use the

expression) is sufficiently large to afford a good view of what passes in the fire-place.

This conical hole is represented in the Figures 18 and 21, by dotted lines. It is situated on the left hand of the entrance into the fire-place. Into the opening of the hole in the wall, on the outside of it, is fixed a short tube of copper, (about six inches in diameter, and four inches long,) and in this tube another short *moveable* tube is fitted, one end of which is closed by the circular plate of glass which constitutes the window. As the wall of the fire-place in front is thick, this pane of glass is at a considerable distance from the burning Fuel, and as there is no draft through the hole in the wall, the glass does not grow very hot.

I have been the more particular in my description of this little invention, as I think it may be useful: There are many cases in which it would be very advantageous to know exactly what is going on in a closed fire-place; and this never can be known by opening the door; for the instant the door is opened, the cold air rushing with impetuosity into the fire-place, deranges entirely the whole economy of the Fire: Besides this, it is frequently very disadvantageous to the process which is going on, to open the door of a fire-place; and it is always attended with a certain loss of heat, and consequently should as much as possible be avoided.

I intimated that the window I have been describing afforded me amusement;—it did still more,—it afforded me much useful information;—it gave me

an opportunity of *observing* the various internal motions into which flame may, by proper management of the machinery of a fire-place, be thrown; and of estimating, with some degree of precision, their different effects. In short, it made me better acquainted with the subject which had so long engaged my attention—(Fire);—and with regard to *that* subject, nothing surely that is new can be uninteresting. But to return to the Brewery:—To the top of the Boiler was fitted a curb of oak timber: The four strait beams of which this curb was constructed are each about 7 inches thick, and 15 inches wide; and the upper part of the Boiler is fastened by large copper nails to the inside of the square frame formed by these four beams. From the top of this curb is raised a wooden building, like the roof of a house with a double slant or bevel, which serves as a cover to the Boiler. This building, the sides of which are about three feet high inwards, and the top of which is covered in by a very flat roof, slanting on every side from the centre,—is constructed of a light frame-work of timber, (four-inch deal joists,) which is covered within as well as without with thin deal boards, which are rabbetted into each other at their edges, to render the cover which this little edifice forms for the Boiler as tight as possible.

From the top of this cover, an open wooden tube, (*m.* Fig. 17.) about 12 inches in diameter, rises up perpendicularly, and going through the roof of the Brewhouse, ends in the open air. This

tube, which is furnished with a wooden damper, is intended to carry off the steam.

On the side of this cover next the mashing-tub, as also on that opposite to it, by which the wort runs off into the coolers, there are large folding wooden doors, (*i* and *k*, Fig. 17.) which are occasionally lifted up by means of ropes which pass over pullies fastened to the ceiling of the Brewhouse.

There are likewise two glass windows (see Fig. 17.) in two opposite sides of the cover, through which, as soon as in consequence of the boiling of the liquid the steam becomes transparent and *invisible*, (which happens in a very few minutes after the liquid has began to boil,) the contents of the Boiler may be distinctly seen and examined.

Whenever there is occasion during the boiling to open either a door or a window of the cover, it is necessary to begin by opening the damper of the steam-chimney, otherwise the hot steam, rushing out with violence, would expose the by-standers to the danger of being scalded; but when the damper of the steam-chimney is open, no steam comes into the Brewhouse, though a door or window of the cover be wide open.

Another similar precaution is sometimes necessary in opening the door of the fire-place, which it may be useful to mention.—When the dampers in the canals by which the smoke goes off into the chimney are nearly closed, (which must frequently be done to confine and economise the heat,) if, without altering the damper, or the register in the
ash-

ash-pit door, the fire-place door be suddenly opened, it will frequently happen that smoke, and sometimes flame, will rush out of the fire-place by this passage. This accident may be easily and effectually prevented, either by opening the damper, or by closing the register of the ash-pit door, the moment before the fire-place door is opened. — This precaution should be attended to in all fire-places of all dimensions, constructed on the principles I have recommended.

To economise the time and the *patience* of my reader as far as it is possible, without suppressing any thing essential relating to the subject under consideration, I shall give him, in a very small compass, the general results of a set of Experiments which cost me more labour (or at least more *time*) than it would cost him to read all the Essays I have ever written. I believe I am sometimes too prolix for the taste of the age, — but it should be remembered that the subjects I have undertaken to investigate are by no means indifferent to me ; — that I conceive them to be intimately connected with the comforts and enjoyments of mankind ; — and that a habit of revolving them in my mind, and reflecting on their extensive usefulness, has awakened my enthusiasm, and rendered it quite impossible for me to treat them with cold indifference, however indifferent or tiresome they may appear to those who have not been accustomed to view them in the same light.

I have

I have already given an account, in all its various details, of one Experiment which was made, (on the 15th of April 1795,) with the Boiler we have just been describing (see page 78). I shall now recapitulate the general results of that Experiment, and compare them with the mean results of two other like Experiments made with the same Boiler.

	Experiment No. 27.	Experiment No. 28.
Quantity of water in the Boiler -	12,508 lbs.	12,508 lbs.
Temperature of the water in the Boiler at the beginning of the Experiment - - - - -	60°	58°
Time required to make the water boil - - - - -	3 h. 40 min.	3 h. 48 min.
Fuel employed to make the water boil - - - - -	800 lbs.	825 lbs.
Time the water continued boil- ing - - - - -	2 h. 43 min.	—
Fuel added to keep the water boiling - - - - -	100 lbs.	—
Kind of Fuel used - - - - -	Pine-wood	Pine-wood
PRECISE RESULTS of the Experiments.		
Quantity of <i>ice-cold water</i> which might be heated 180°, or made to boil, with the heat gene- rated in the combustion of 1 lb. of the Fuel - - - - -	12.06 lbs.	12.70 lbs.
Time in which, according to the result of the Experiment, <i>ice- cold water</i> might (at Munich) be made to boil with the given proportion of Fuel - - - -	4 h. 20 min.	4 h. 20 min.
Quantity of <i>boiling-hot water</i> kept boiling one hour with the heat generated in the com- bustion of 1 lb. of the Fuel -	339.80 lbs.	—

On

On comparing the results of these Experiments with those made in the Boilers of the kitchens of the House of Industry and Military Academy, I was led to imagine that either the Boiler, or the fire-place of the Brewery, or both, were capable of great improvement; for in some of the Experiments with these small kitchen Boilers, the Economy of Fuel had been carried so far, that with the heat generated in the combustion of 1 lb. of pine-wood, it appeared that 20 lbs. of ice-cold water might have been made to boil;—but here, though the machinery was on a scale so much larger, (and I had concluded, too rashly indeed, as will be shown hereafter, that the larger the Boiler the greater is of course the Economy of Fuel,)—the results of these Experiments indicated, that not quite 13 lbs. of ice-cold water could have been made to boil with the heat furnished in the combustion of 1 lb. of the wood.

The Experiments, No. 22, No. 25, and No. 26, which were made with the largest of my kitchen Boilers, had, it is true, afforded grounds to suspect that, beyond certain limits, an increase of size in a Boiler does not tend to diminish the expence of Fuel in the process of heating water; yet, as all my other Experiments had tended to confirm me in the opinion I had, at an early period, imbibed on that subject, I was disposed to suspect any other cause than the true one of having been instrumental in producing the unexpected appearances I observed.

I was

I was much disappointed, I confess, at finding that the Brewhouse Boiler, notwithstanding all the pains I had taken to fit up its fire-place in the most perfect manner, and notwithstanding its enormous dimensions, when compared with the Boilers I had hitherto used in my Experiments, so far from answering my expectations, actually required considerably more Fuel, in proportion to its contents, than another Boiler fitted up on the same principles, which was not *one fiftieth* part of its size.

This unexpected result puzzled me;—and I must own that it vexed me, though I ought perhaps to be ashamed of my weakness;—but it did not discourage me. Finding on examining the Boiler, that its bottom was very thick, compared with the thickness of the sheet copper of which my Kitchen Boilers were constructed, it occurred to me that possibly *that* might be the cause, or at least *one of the causes*, which had made the consumption of Fuel so much greater than I expected; and as there was another Brewhouse in the neighbourhood belonging to the Elector, which, luckily for me, stood in need of a new Boiler, I availed myself of that opportunity to make an Experiment, which not only decided the point in question, but also established a new fact with regard to heat, which I conceive to be of considerable importance.

Having obtained the Elector's permission to arrange the second Brewhouse as I should think best, I determined to spare no pains to render it as perfect as possible in all respects, and particularly in
every

every thing relating to the Economy of Fuel. As in brewing, in the manner that business is carried on in Bavaria; where the whole process, in as far as Fire is employed in it, is begun and finished in the course of a day, *the saving of time*, in heating the water and boiling the wort, is an object of almost as much importance as that of economizing Fuel, and consequently demanded particular attention.

The means I used for the attainment of both these objects will be evident from the following description of the Boiler and its fire-place, which I caused to be constructed, and which are represented in all their details in the Plates III, IV, and V.

This Boiler is 12 (Bavarian) feet long, 10 feet wide, and only 2 feet deep. The sheet copper of which it is made is uncommonly thin for a Boiler of such large dimensions, being at a medium less than *one-tenth* of an English inch in thickness. This Boiler, when finished, weighed no more than 674 lbs. Bavarian weight, equal to 834½ lbs. Avoirdupois, exclusive of 64 lbs. of copper nails used in rivetting the sheets of copper together.

The top of the Boiler is surrounded by a strong curb (*a, b*, Fig. 17.) of oak timber, to which it is attached by strong copper nails, and over the Boiler is built a roof, or standing cover, (see Fig. 17.) similar in all respect to that already described. The bottom of the Boiler is flat, and reposes horizontally on the top of the thin brick walls by which the fire-place is divided into flues. (See Fig. 18.)—These flues do not run in the direction of the length of

of the Boiler, but from one side of it to the other;—consequently the door of the fire-place is in the middle of one side of the Boiler.

The sheets of copper, of which the bottom of the Boiler was constructed, run in the direction of the flues; and they are just so wide that their seams or joinings (where they are united to each other by their sides) repose on the walls of the flues, except only in the middle flue, which, being about twice as wide as the others, one seam was necessarily left unsupported, at least a considerable part of its length.—The sheets of copper used in constructing this part of the bottom of the Boiler are rather thicker and stronger than the rest: They are just 0.118 of an English inch in thickness.

The Fire is made under this Boiler in the middle flue, which, as I have just observed, is a little more than twice as wide as one of the other flues. There are *five* flues under the Boiler; namely, one in the middle 44 inches wide, above, in the clear, (which constitutes the fire-place,)—and *two* on each side of it, in which the flame circulates; one 20 inches wide, and the other 19 inches wide.

The side flues are each $14\frac{1}{2}$ inches deep;—but as the walls which separate them are much thicker below than above, where the bottom of the Boiler reposes on them, the width of these flues below is only 13 inches.—The walls of these flues are shown by dotted lines in Fig. 17.

The walls which separate the flues do not run quite from one side of the Boiler to the other; an opening

opening being left at one end of each of them equal to the width of one of the narrow flues for the passage of the flame from one flue into another, without its going from under the Boiler.

The Fire being made (on a circular grate) in the middle flue (see Fig. 18.), the flame passes on in this flue to its farther end, and then, dividing to the right and left, comes forward in the two adjoining side-flues. Having arrived at the wall which supports the front of the Boiler, it turns again to the right and left; and, entering the two outside flues, returns in them to the back of the Boiler. Here it went out (before the fire-place was altered) at two openings left for that purpose in the wall which supports the back part of the Boiler, and the two currents of flame uniting, entered a canal 7 inches wide, and 16 inches high, which goes all round the outside of the Boiler. (See Fig. 20.) Having made the circuit of the Boiler, it went off by a canal (furnished with a damper) into the chimney.

From this description of the fire-place, it appears that the flame and smoke generated in the combustion of the Fuel in passing through those different flues, made a circuit of above 70 feet, in contact with the surface of the Boiler, before they were permitted to escape into the chimney. This I thought must be sufficient to give these hot fluids an opportunity of communicating to the Boiler all the heat they could part with, notwithstanding the difficulties which attend their getting rid of it: And I concluded that the communication of their heat

to the Boiler would be much facilitated and expedited by the various eddies and whirlpools produced in the flame in consequence of the number of abrupt turns and changes of direction it was obliged to make in passing under and round the Boiler.

As the Experiments which have been made with this Boiler were conducted throughout with the utmost care and attention, and as their results are both curious and important in several respects, I have thought them deserving of being made known to the Public in all their details.

An Account of three Experiments made at Munich the 10th October 1796, with the new Boiler in the Brewery called *Neubeusel*, belonging to HIS MOST SERENE HIGHNESS the ELECTOR.—The weather being fair: The barometer standing at 28 English inches, and Fahrenheit's thermometer at 36°.

Dimensions of the Boiler, in English measure, as found by actual measurement.	} Length	11 feet 6.02 inches,	
		} Width	9 — 7.723 —
			Depth

Contents of the Boiler, when quite full to the brim, 14,163 lbs.
Bavarian weight of water, at the temperature of 55°, equal
to 17,540 lbs. Avoirdupois, or 2099 wine gallons.

The Boiler actually contained of water, in the beginning of each of the two following Experiments, - - -	} in <i>Bavarian weight</i> ,	—8120 lbs.
		equal to 10,056 lbs. Avoirdupois, or nearly 1204 wine gallons.

The wood used in this and the following Experiments was *Pine*, which had been moderately seasoned, and the billets were 3 feet 4½ inches, English measure, in length.

FIRST EXPERIMENT WITH THE NEW BOILER.

Experiment, No. 29.

Time.		Fire-wood put into the Fire-place.		Temperature of the water in the Boiler.
Hours.	Min.	Number of Billets.	Quantity in weight. lbs.	In degrees of Fahrenheit's Therm.
11	31 A.M.	10	50	50°
—	46	15	25	54
12	0	5	25	64
—	10 P.M.	5	25	67
—	36	—	—	85
—	40	4	25	—
—	53	5	25	96
1	12	7	25	105
—	21	10	50	110
—	46	10	50	129
—	58	40	50	—
2	17	46	50	156
—	29	—	—	164
—	34	10	50	—
—	41	—	—	173
—	49	—	—	180
—	58	40	50	185
3	15	12	50	197
—	26	20	25	205
3	35	—	—	the water boiled.

Time employed, } 4 h. 4 min. Wood consumed, 575 lbs.

The boiling water being let off, and it being replaced immediately with cold water, the Experiment was repeated as follows :

Experiment, No. 30.

Time.		Quantity of Fire-wood put into the Fire place.		Temperature of the water in the Boiler.
Hours.	Min.	Number of Billets.	Quantity in weight. lbs.	In degrees of Fahrenheit's Therm.
4	41 P. M.	40	50	60 ⁵ / ₁
—	50	40	50	72
5	4	10	50	86
—	16	10	50	99 ¹ / ₂
—	29	10	50	114
—	42	10	50	126
—	56	40	50	142
6	10	40	50	157
—	24	40	50	172
—	28	—	—	172
—	40	40	50	185 ¹ / ₂
—	42 ¹ / ₂	—	—	185 ¹ / ₂
—	53	40	50	198
—	55	—	—	205
7	2	—	—	the water boiled.
7	7	—	—	

Time employed, } 4 h. 26 min. Wood consumed, 550 lbs

This boiling water being let off, the Boiler was again filled (immediately) with cold water; and in this third Experiment the quantity of water was increased to 11,368 lbs. *Bavarian weight*,—equal to 14,078 lbs. *Avoirdupois*, or 1685 wine gallons.

The results of this Experiment were as follows;

Experiment, No. 31.

Time.		Quantity of Fire-wood put into the Fire-place.		Temperature of the water in the Boiler.
Hours.	Min.	Number of Billets.	Quantity in weight, lbs.	In. degrees of Fahrenheit's Therm.
8	51 P. M.	80	100	65½
9	7 —	40	50	79°
—	21 —	40	50	90
—	44 —	40	50	107
—	57 —	40	50	118
10	14 —	40	50	130
—	28 —	40	50	140
—	45 —	40	50	155
11	— —	40	50	165
—	15 —	40	50	175
—	30 —	40	50	182
—	45 —	40	50	200
11	58. —	—	—	the water boiled.

Time employed, } 3 h. 7 min. Wood consumed, 650 lbs.

I was surprised when I compared the results of these Experiments with those made in the other
The

The following Table will show the results of these three Experiments in a clear and satisfactory manner:

	Experiment No. 29.	Experiment No. 30.	Experiment No. 31.
Quantity of water in the Boiler at the beginning of the Experiment, in <i>Bavarian pounds</i> - - -	8120 lbs.	8120 lbs.	11,368 lbs.
Temperature of the water at the beginning of the Experiment -	50°	60°	65½°
Time employed in making the water boil -	4 h. 4 min.	2 h. 26 min.	3 h. 7 min.
Fuel (Pine wood) consumed in making the water boil, in <i>Bavarian pounds</i> - - -	575 lbs.	550 lbs.	650 lbs.
<i>Precise Results of the Experiments.</i>			
Quantity of ice-cold water which might have been heated 180 degrees, or made to boil with the heat generated in the combustion of 1 lb. of the Fuel -	12.54 lbs.	12.28 lbs.	14.59 lbs.
Time in which, according to the result of the Experiment, ice-cold water might be made to boil at Munich with the given proportion of Fuel - - - -	4 h. 31 min.	2 h. 59 min.	3 h. 35 min.

I was surprised, when I compared the results of these Experiments with those made in the other Brewhouse,

Brewhouse, to find how little in appearance I had gained by the alterations I had introduced; on a more careful examination of the matter, however, I found that I had gained much more than I at first imagined, both in respect to the Economy of Fuel, and to that of Time. The amount of these advantages will appear from the following comparison of the mean result of these two sets of Experiments:

<i>Precise Results of the foregoing Experiments.</i>		
	Quantity of ice-cold water made to boil with 1 lb. of the Fuel.	Time required to make ice-cold water boil, according to the result of the given Experiment.
<i>First Set.</i>		
In the Experiment No. 27. —	lbs. 12.06	hrs. min. 4 20
In the Experiment No. 28, —	12.70	4 20
Sum —	24.77	8 40
Means —	12.385	4 20
<i>Second Set.</i>		
In the Experiment No. 29. —	12.54	4 31
In the Experiment No. 30. —	12.28	2 59
Sum —	24.82	7 30
Means —	12.41	3 45

The mean results of these two sets of Experiments differ very little from each other in appearance;

ance; and from this circumstance I shall prove, that the new Boiler is better adapted for saving Fuel than the old.

By comparing the results of the Experiments made with the same Boiler, but with different quantities of water, we shall constantly find that the expence of Fuel was *less* in proportion as the quantity of water was *greater*. In the Experiment No. 23, when 127 lbs. of water were used, the result of the Experiment indicated that no more than 12.74 lbs. of ice-cold water could be made to boil with the heat generated in the combustion of 1 lb. of the Fuel used; but in the Experiment No. 26, made with the same Boiler, but when 4 times as much water was used, or 508 lbs. it appeared from the result of the Experiment, that 19.01 lbs. of ice-cold water might be made to boil with 1 lb. of the Fuel.

Now, in the first set of the Experiments we are comparing, as the quantity of water used (12,508 lbs.) was much greater than that used in the second set (8120 lbs.), it is evident, that if the construction of the machinery and the Management of the Fire had been equally perfect in the two cases, the Economy of Fuel would have been greatest where the largest quantity of water was used;—that is to say, in the first set of Experiments;—but, as that was not the case, it is certain that the Boiler used in the second set is better adapted to economize Fuel than that used in the first.

But we need not go so far to search for proofs of that fact. The result of the Experiment No. 31 is

is alone sufficient to put the matter beyond doubt. In this Experiment, in which the quantity of water (though still considerably short of that used in the former set of Experiments) was augmented from 8120 lbs. to 11,368 lbs. the saving of Fuel was so much increased as to show in a decisive manner the superiority of the new Boiler.

	Quantity of ice-cold water to made to boil with 1 lb. of the Fuel.		Time required to make ice- cold water boil, according to the result of the Experiment.	
	lbs.		hrs.	min.
<i>The Precise Results</i>				
Of this Experiment (No. 31.) were as follows, — — —	14.59		3	37
In the Experiments No. 27 and No. 28, they were, at a medium,	12.385		4	20

The difference in the expence of Fuel in these Experiments with these two Boilers is by no means inconsiderable; it amounts to above 14 per cent. and would have amounted to more, if more time had been allowed for heating the water in the Experiment with the new Boiler; for it is easy to show — (what indeed was clearly indicated by all the Experiments) — that, in causing liquids to boil, the quantity of Fuel will be less in proportion as the time employed in that process is long; or, which is the same, — as the Fire is smaller: And the saving of Fuel arising from any given prolongation of the process, will be the greater as the fire-place is more perfect, and as the means used for confining the heat are more effectual.

Though the general results of these two sets of Experiments afforded abundant reason to conclude that the alterations I had introduced in arranging the new Boiler were real improvements; yet, when I compared the quantity of Fuel consumed in the Experiments with this new Boiler, with the much smaller quantities, in proportion to the quantity of water, which were employed in some of my former Experiments with kitchen Boilers, I was for some time quite at a loss to account for this difference. In all my Experiments with Boilers of different sizes, from the smallest saucepan up to the largest kitchen Boilers, I had invariably found that the *larger* the quantity of water was which was heated, the *less*, in proportion, was the quantity of Fuel necessary to be employed in that process; and so entirely had that prejudice taken possession of my mind, that when the strongest reasons for doubt presented themselves, they were overlooked; and it was not till I had searched in vain on every side to discover some other cause to which I could attribute the unexpected appearance that embarrassed me, that I was induced,—I may say forced,—to abandon my former opinion, and to be convinced that what I had too hastily considered as a general law, does not in fact obtain but within narrow limits;—that although, in heating *certain qualities* of liquids, there is an advantage in point of the Economy of Fuel in performing the process on a larger scale, in preference to a smaller one; yet, when the liquid to be heated amounts to a certain quantity,

quantity, this advantage ceases; and if it exceeds that quantity, it is attended with an expence of Fuel proportionally greater than when the quantity is less.

What the size of a Boiler must be, in order that the saving of Fuel may be a *maximum*, I do not pretend to have determined. I think however that there are some reasons for suspecting that it would not be larger than some of the kitchen Boilers used in my Experiments. But I recollect to have promised my Reader, that I would not give him my opinion, without laying before him at the same time the grounds of those opinions.—In the present case they are as follows:

In an Experiment of which I have already given an account (No. 3.), $7\frac{1}{2}$ lbs. of water, at the temperature of 58° , were made to boil in a saucepan fitted up in my best manner, in a closed fire-place; and the wood consumed was 1 lb. This gives for the *precise result* of the Experiment, 6.68 lbs. of ice-cold water made to boil with 1 lb. of the Fuel.

In another Experiment (No. 12.) made with one of the small Boilers belonging to the kitchen of the Military Academy, fitted up on the same principles, 43.63 lbs. of water, at the temperature of 60° , were made to boil with 3 lbs. of wood. This gives 11.93 lbs. of ice-cold water made to boil with 1 lb. of the Fuel.

Again, in the Experiment No. 20, which was made with a larger Boiler belonging to the same kitchen,

Though the general results of these two sets of Experiments afforded abundant reason to conclude that the alterations I had introduced in arranging the new Boiler were real improvements; yet, when I compared the quantity of Fuel consumed in the Experiments with this new Boiler, with the much smaller quantities, in proportion to the quantity of water, which were employed in some of my former Experiments with kitchen Boilers, I was for some time quite at a loss to account for this difference. In all my Experiments with Boilers of different sizes, from the smallest saucepan up to the largest kitchen Boilers, I had invariably found that the *larger* the quantity of water was which was heated, the *less*, in proportion, was the quantity of Fuel necessary to be employed in that process; and so entirely had that prejudice taken possession of my mind, that when the strongest reasons for doubt presented themselves, they were overlooked; and it was not till I had searched in vain on every side to discover some other cause to which I could attribute the unexpected appearance that embarrassed me, that I was induced,—I may say forced,—to abandon my former opinion, and to be convinced that what I had too hastily considered as a general law, does not in fact obtain but within narrow limits;—that although, in heating *certain qualities* of liquids, there is an advantage in point of the Economy of Fuel in performing the process on a larger scale, in preference to a smaller one; yet, when the liquid to be heated amounts to a certain quantity,

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In an Experiment of which I have already given an account (No. 3.), $7\frac{1}{8}$ lbs. of water, at the temperature of 58° , were made to boil in a saucepan fitted up in my best manner, in a closed fire-place; and the wood consumed was 1 lb. This gives for the *precise result* of the Experiment, 6.68 lbs. of ice-cold water made to boil with 1 lb. of the Fuel.

In another Experiment (No. 12.) made with one of the small Boilers belonging to the kitchen of the Military Academy, fitted up on the same principles, 43.63 lbs. of water, at the temperature of 60° , were made to boil with 3 lbs. of wood. This gives 11.93 lbs. of ice-cold water made to boil with 1 lb. of the Fuel.

Again, in the Experiment No. 20, which was made with a larger Boiler belonging to the same kitchen,

kitchen, and fitted up in the same manner, 187 lbs. of water, (equal to about 28 gallons,) at the temperature of 55° , were made to boil with the combustion of 8 lbs. of fire-wood. This gives 20.10 lbs. of ice-cold water made to boil with 1 lb. of the wood;—and farther than this I have not been able to push the Economy of Fuel.

In the Experiment No. 26. a Boiler was used, which had been constructed with the express view to see how far it was possible to carry the Economy of Fuel in culinary processes; and it was fitted up with the utmost care, and on the most approved principles. As I thought at that time that a large-sized Boiler was essential to the economizing of Fuel, this Boiler was made to contain 106 gallons. In the Experiment in question it actually contained 508 Bavarian pounds of water, (or about 63 gallons,) at the temperature of 48° ; and to make this water boil, 24 lbs. of wood were consumed. This gives 19.01 lbs. of ice-cold water made to boil with 1 lb. of Fuel. Hence it appears that the expence of Fuel was greater in this Experiment than in that last-mentioned.

Again, in the Experiment No. 31. when no less than 11,368 lbs. or 1685 gallons, of water were heated and made to boil in the new Brew-house Boiler; the wood consumed amounted to 650 lbs. which (as the temperature of the water at the beginning of the Experiment was $65\frac{1}{2}^{\circ}$) gives for the *precise result* of the Experiment, 14.59 lbs. of ice-cold water made to boil with the heat generated in the combustion of 1 lb. of the Fuel.

As

As the relative quantities of Fuel expended in the Experiments are inversely as the numbers expressing the quantities of ice-cold water, which, from the result of each Experiment, it appears might have been heated 180 degrees, or made to boil, under the mean pressure of the atmosphere at the level of the sea, with the heat generated in the combustion of 1 lb. of the Fuel; it is evident that these numbers measure very accurately the different degrees to which the Economy of Fuel was carried in the different Experiments. The Economy of Fuel in heating liquids depending on the quantity of the liquid, as shown by the foregoing Experiments, may therefore be expressed shortly in the following manner:

	Quantity of water heated in the Experiment, in Bavarian lbs.	Degrees to which the Economy of the Fuel was carried.
In the Experiment No. 3,	7.93	6.68
No. 12,	43.63	11.93
No. 16,	187	20.10
No. 26,	503	19.01
No. 31,	11,368	14.59

Before I take my leave of this subject I would just remark, that the cause of the appearances observed in the Experiments may, I think, be traced to that property of flame from which it has been denominated a non-conductor of heat:—For if the different particles of flame give off their heat only

to bodies with which they actually come into contact, the quantity of heat given off by it will be,—not as its volume, (and consequently not as the quantity of Fuel consumed,) but rather as its surface. And as the surface of the flame, when fire-places are similar, is proportionally greater in small than in large fire-places;—the surfaces of similar bodies being as the *squares* of their corresponding sides, while their volumes are as the *cubes* of those sides;—it is evident that, on that account, less heat in proportion to the quantity generated in the combustion of the Fuel ought to be communicated to the Boiler, when the fire-place and Boiler are large, than when the process is carried on upon a smaller scale.

There are, however, several other circumstances to be taken into the account in determining the effects of *size* in the machinery necessary for boiling liquids; and one of them, which has great influence, is the heat absorbed by the masonry of the fire-place. This loss will most undoubtedly be the smaller, as the fire-place is larger; but to determine the exact point when, the saving on the one hand being just counterbalanced by the loss on the other, any augmentation or diminution of size in the machinery would be attended with a positive loss of heat, is not easy to be ascertained. Provided however that proper attention be paid to the *Management of the Fire*, and that as much heat as possible be generated in the combustion of the Fuel—(which may always be done in the largest fire-place

place as well, if not better, than in smaller ones); as that part of the heat which goes off in the smoke is, indubitably lost, a thermometer placed in the chimney would indicate, with a considerable degree of precision, the perfections or imperfections of the fire-place.

It is well known that the smoke which rises from the chimnies of the closed fire-places of very large Boilers is much hotter than that which escapes from smaller fire-places; and I am surpris'd that this fact, which has long been known to me, should not have led me to suspect that the waste of Fuel was proportionally greater in these large fire-places than in smaller ones.

Besides the Experiments of which I have given an account, several others were made with the new Brewhouse Boiler; and, among others, four Experiments were made on four succeeding days, in brewing Beer; and it was found that considerably less Fuel was expended in these trials, than was necessary in brewing the same quantity of beer in the other Brewhouse, in which I first introduced my improvements. But though the alteration of form, diminution of the thickness of the metal, &c. which I had introduced in constructing the new Boiler, and also in the manner of fitting it up, had produced a considerable saving of Fuel, yet it was not accompanied by a proportional saving of time. I had flattered myself that by making the Boiler *very thin* and *very shallow*, I should bring its contents to boil in a *very short time*; but I did not con-

sider how much time is necessary for the combustion of the Fuel necessary for heating so large a quantity of water; otherwise my expectations on this head would have been less sanguine. The quantity of heat generated in any given time being as the quantity of Fuel consumed, it must depend in a great measure on the size of the fire-place; and when it is required to heat a large quantity of water, or of any liquid, in a very short time, either the fire-place must be large, or (what in my opinion would be still better) a number of separate fire-places,—two or three, for instance,—must be made under the same Boiler. The Boiler should be made wide and shallow, in order to admit of a great number of flues, in which the flame and smoke of the different Fires should be made to circulate separately under its bottom.

The combustion of the Fuel, and consequently the generation and communication of the heat, may in the same fire-place be considerably accelerated by increasing the draught (as it is called) of the Fire; which may be done by increasing the height of the chimney, or by enlarging the canal leading to the chimney, and keeping the damper open, when that passage is too small;—or by shortening the length of the flues.

The master brewer having expressed a wish that some contrivance might be used by which the water might be made to boil a little sooner in the new Boiler, I made an alteration in its fire-place which completely answered that purpose,

But besides the desire I had to oblige the master brewer, (who only thought how he could contrive to finish as early as possible his day's work,) I had another, and much more important object in view. Having had reason to suspect that flues which go round on the outside of large Boilers do little more than prevent the escape of the heat by their sides, —which, with infinitely less trouble and less expence, may be prevented by other means,—I was desirous of finding out, by a decisive Experiment, the real amount of the advantages gained by those flues; or the saving of Fuel which they produce. And as I was confident that the suppression of the flue which went round the new Boiler would increase the draught of the fire-place, and accelerate the combustion of the Fuel, I concluded that if my opinion was well founded with respect to the smallness of the advantages derived from these *side flues*, the increase of heat arising from the acceleration of the combustion occasioned by the increased draught on closing them up would more than counterbalance the loss of those advantages, and the time employed in heating the water would be found to be actually less than it was before.

The results of the following Experiments show how far my suspicions were founded.

Experiment, No. 32.

The flue round the outside of the new Brewhouse Boiler having been closed up, and two canals (*a* and *b*, Fig. 21.) formed from the end of the two
outside

outside flues of those situated *under* the Boiler, by which two canals (which were both furnished with dampers) the smoke passed off from under the Boiler directly into the chimney; the Experiment No. 31. which was made with the same Boiler before the outside flues were closed up, was now repeated with the utmost care, in order to ascertain the effects which the closing up of those flues would produce. The quantity of water in the Boiler, and its temperature at the beginning of the Experiment, were the same;—the wood used as Fuel was taken from the same parcel, and it was put into the fire-place in the *same quantities*, and at the *same intervals of time*;—in short, every circumstance was the same in the two Experiments, excepting only the alterations which had been made in the fire-place. As the length of the flues through which the flame and smoke was obliged to pass to get into the chimney had been diminished more than half, (or reduced from 70 to about 30 feet,) the strength of the draught of the fire-place was much increased, as was evident not only from the increased violence of the combustion of the Fuel, which was very apparent, but also from another circumstance, which I think it my duty to mention. Before the flue round the Boiler was closed, if too much Fuel was put into the fire-place at once, it not only did not burn with a clear flame, but frequently the smoke, and sometimes the flame, came out of the fire-place door, even when the damper in the chimney was wide open; but after this flue was closed up, it was found to be hardly

hardly possible to overcharge the fire-place, and the Fuel always burned with the utmost vivacity.

I ought to inform my Reader, that though the entrance into the flue which went round the outside of the Boiler was closed, and another and a shorter road opened for the flame and smoke to pass off into the chimney, yet the cavity of the flue remained; and by means of openings (c. d. c. c. c. Fig. 21. Plate V.) about six inches square in the brick-work which separated this old road (which was now shut up) from the flues *under the Boiler*, the flame was permitted to pass into this cavity, and to spread itself round the outside of the Boiler. This contrivance (which I would recommend for all Boilers) not only prevents the escape of the heat out of the Boiler by its sides, but contributes something towards heating it; and as the openings in the sides of the flues do not sensibly impede the motion of the flame, they can do no harm.

As the two Experiments, the results of which I am about to compare, were made with the greatest care, and as they are on several accounts uncommonly interesting, I shall place them in a conspicuous point of view.

Having found by comparing the results of these two Experiments that I had lost nothing in respect to the Economy of Fuel, by shutting up the outside flue of my Boiler, I was now desirous of knowing how much I had gained in point of time.

A COMPARATIVE VIEW of TWO EXPERIMENTS made with a new Brewhouse Boiler.

The time is reckoned from the beginning of the Experiment, and was the same in both Experiments.

Quantity of water in the Boiler 11,368 lbs. Bavarian weight.

Time from the begin- ning of the Experiment.	Fuel put into the fire-place.		Heat of the water in the Boiler.	
	Number of billets.	Quantity in weight.	Experiment No. 31, (outside flue open).	Experiment No. 32, (outside flue closed).
hrs. min.	No.	lbs.	degrees.	degrees.
— —	80	100	65 $\frac{1}{2}$	65 $\frac{1}{2}$
0 16	40	50	79	82
0 30	40	50	90	94
0 53	40	50	107	110
1 6	40	50	118	122
1 23	40	50	130	135
1 37	40	50	140	147
1 54	40	50	155	160
2 9	40	50	165	171
2 24	40	50	175	182
2 39	40	50	182	191
2 54	40	50	200	—
2 59	—	—	—	boiled
3 7	—	—	boiled	—

Having found by comparing the results of these two Experiments, that I had lost nothing in respect to the Economy of Fuel, by shutting up the outside flue of my Boiler, I was now desirous of ascertaining how much I had gained in point of time,

or

or how much the increased draught of the fire-place, in consequence of its flues being shortened, enabled me to abridge the time employed in causing the contents of the Boiler to boil, in cases in which it should be advantageous to expedite that process at the expence of a small additional quantity of Fuel.

By the following Experiment, in which the combustion of the Fuel was made as rapid as possible by keeping the fire-place full of wood, and the register in the ash-pit door and the damper in the chimney constantly quite open, may be seen how far I succeeded in the attainment of that object.

[illegible]

the Experiment No. 2, exactly at the temperature of water at the temperature of 62°, was made to boil in a hour and a half, with the consumption of 62 lbs. of the same kind of wood. Had the water in this Experiment been as cold as it was in the Experiment No. 2, exactly at the temperature of

Experiment No. 33.

The Boiler contained 11,368 lbs. Bavarian weight of water at the temperature of 47° . The Fuel used was pine-wood, moderately seasoned, in billets three feet four inches long, and split into small pieces of about 1 lb. each, that it might burn the more rapidly.

This Experiment was made the 29th of November 1796, the barometer standing at 26 inches 8.7 lines Paris measure, and Fahrenheit's thermometer at 33° .

Time.	Fuel put into the fire-place.	Tempera- ture of the water in the copper.
hrs. min.	lbs.	degrees.
2 0	100	47
0 14	100	58
0 34	100	88
0 51	100	100
3 9	100	123
0 25	100	144
0 39	100	151
4 0	100	—
0 10	—	200
0 17	—	boiled
Time employed - 2 17		Wood consumed - 800

In the Experiment No. 32, the same quantity of water at the temperature of $65\frac{1}{2}^{\circ}$, was made to boil in 2 hours 59 minutes, with the consumption of 625 lbs. of the same kind of wood. Had the water in this Experiment been as cold as it was in the Experiment No. 33, (namely at the temperature

ture of 47° .) instead of 625 lbs., 705 lbs. of the Fuel would have been necessary; and the process, instead of lasting 2 hours and 59 minutes, would have lasted 3 hours and 22 minutes.

Hence we may conclude, that to abridge 1 hour and 5 minutes of 3 hours and 22 minutes in the process of boiling 11,368 lbs. of water, this cannot be done at a less additional expence of Fuel than that of 95 lbs. of pine-wood;—or, to abridge the time *one-third*, there must be an additional expence of about *one-eighth* more Fuel.

In some cases it will be most profitable to save time, in others to economise Fuel;—and it will always be desirable to be able to do either, as circumstances may render most expedient.

From a comparison of the quantities of Fuel consumed, and consequently of heat generated, in the same time, with the quantities of heat actually communicated to the water, in the Experiments No. 32 and No. 33, during this time, an idea may be formed of the great quantity of heat that may remain in flame and smoke after they have passed many feet in flues under the thin bottom of a Boiler containing cold water; and this shows with how much difficulty these hot vapours part with their heat, and how important it is to be acquainted with that fact in order to take measures with certainty for economising Fuel.

I have been the more particular in my account of these Experiments with large Boilers, as I believe no Experiments of the kind on so large a scale have been yet made; and as they were all conducted

with care, their results have intrinsic value independent of the particular uses to which I have applied them.

As in the countries where this Essay is likely to be most read, pit-coals are more frequently used as Fuel than wood, it will not only be satisfactory, but in many cases may be really useful to my Reader to know the relative quantities of heat producible from coals and from wood; in order to be able to compare the results of Experiments in which coals are used as Fuel, with those of which I have here given an account; or to determine the quantity of coals necessary in any process which it is known may be performed with a given quantity of wood.

It was my intention to have made a set of Experiments on purpose to determine the relative quantities of heat producible from all the various kind of combustible bodies which are used as Fuel; and I made preparations for beginning them, but I have not yet been able to find leisure to attend to the subject.

The most satisfactory account I have been able to procure respecting the matter in question, is one for which I am indebted to my friend Mr. KIRWAN. By this account, which he tells me is founded on Experiments made by Mr. *Lavoisier*, it appears, that equal quantities of water, under equal surfaces, may be evaporated, and consequently equal heats produced,

By 403 lbs. of Coaks	} or in measure {	By 17 of Coaks
600 — of Pit-coal		10 of Pit-coal
600 — of Charcoal		40 of Charcoal
1089 — of Oak		33 of Oak.

I wish

I wish I were at liberty to transcribe the ingenious and interesting observations which accompanied this estimate; but as they make part of a work which I understand is preparing for the Press, I dare not anticipate what Mr. KIRWAN will himself soon lay before the Public.

According to this estimate it appears that 1089 lbs. of oak produces as much heat in its combustion as 600 lbs. of pit-coal. Now, if we suppose that the pine-wood used in my Experiments is capable of producing as much heat *per pound* as oak,—and I have reason to think it does not afford less,—from the quantity of pine-wood used in any of my Experiments, it is easy to ascertain how much coal would have been necessary to generate the same quantity of heat; for the weight of the coal which would be required is, to the weight of the wood actually consumed, 600 to 1089.

In one of my Experiments (No. 31,)—11,368 lbs. of water, at the temperature of $65\frac{1}{2}^{\circ}$, were made to boil with 650 lbs. of pine-wood. As, when the Experiment was made, the mercury in the barometer stood at about 28 English inches, the temperature of the water when it boiled was only $209\frac{1}{2}^{\circ}$, consequently its temperature was raised ($209\frac{1}{2} - 65\frac{1}{2}$) 144 degrees. Had the water been boiled in London, or in any other place nearly on a level with the surface of the sea, it must have been heated to 212° to have been made to boil,

consequently its temperature must have been raised $146\frac{1}{2}^{\circ}$; and to have done this, instead of 650 lbs. of wood, $661\frac{1}{2}$ lbs. would have been required; (144° is to $146\frac{1}{2}$ as 650 lb. to $661\frac{1}{2}$ lbs.)

If pit-coal were used instead of wood, $363\frac{1}{2}$ lbs. of that kind of Fuel would have been sufficient;— for the quantities of different kinds of Fuel required to perform the same process being inversely as the quantities of heat which equal weights of the given kinds of Fuel are capable of generating, or directly as the quantities of the kind of Fuel in question, which are required to produce the same heat, it is 1089 to 600, as $661\frac{1}{2}$ lbs. of wood to $363\frac{1}{2}$ lbs. of coal, supposing the foregoing estimate to be exact.

Whether it would be possible to cause so large a quantity of water, (1681 wine gallons,) at the given temperature, ($65\frac{1}{2}^{\circ}$), to boil, with this small quantity of coal, I leave to those who are conversant in Experiments of this kind to determine.

From the result of my 20th Experiment it appeared that $20\frac{1}{10}$ lbs. of ice-cold water might be heated 180 degrees, or made to boil under the mean pressure of the atmosphere at the level of the surface of the ocean, with the heat generated in the combustion of 1 lb. of pine-wood. Computing from the result of this Experiment, and from the relative quantities of heat, producible from pine-wood, and from pit-coal, it appears that the heat generated in the combustion of 1 lb. of pit-coal, would make $36\frac{3}{10}$ lbs. of ice-cold water boil.

Hence

Hence it appears that pit-coal should heat 36 times its weight of water, from the freezing point to that of boiling ;—and as it has been found by Experiments made with great care by Mr. WATT, that nearly $5\frac{1}{2}$ times as much heat as is sufficient to heat any given quantity of ice-cold water to the boiling point, is required to reduce that same quantity of water, *already boiling-hot*, to steam; according to this estimation, the heat generated in the combustion of 1 lb. of coal should be sufficient to reduce very nearly 7 lbs. of boiling-hot water to steam.

How far these estimates agree with the Experiments that have been made with steam-engines, I know not; but there seems to be much reason to suspect that the expence of Fuel, in working those engines, is considerably greater than it ought to be, or than it would be, were the Boilers and Fire-places constructed on the best principles, and the Fire properly managed.

In attempts to improve, it is always very desirable to know exactly what progress has been made;—to be able to measure the distance we have laid behind us in our advances; and also that which still remains between us and the object in view. The ground which has been gone over is easily measured; but to estimate that which still lies before us is frequently much more difficult.

The advances I have made in my attempts to improve fire-places, for the purpose of economising Fuel, may be estimated by the results of the
Experiments

Experiments of which I have given an account in this Essay; but it would be satisfactory no doubt to know how much farther it is possible to push the Economy of Fuel.

In my 4th Experiment, $7\frac{1}{8}$ lbs. of water, at the temperature of 58° , were made to boil, at Munich, with 6 lbs. of wood. If, from the result of this Experiment, we compute the quantity of ice-cold water which, with the heat generated in the combustion of 1 lb. of the Fuel, might be heated 180 degrees, or made to boil, it will turn out to be only $1\frac{1}{7}$ lb. or more exactly 1.11 lb.

According to the result of the Experiment No. 20, it appeared, that no less than $20\frac{1}{8}$ lbs. of ice-cold water might have been made to boil with the heat generated in the combustion of 1 lb. of pine-wood.

It appears, therefore, that about *eighteen times* as much Fuel, in proportion to the quantity of water heated, was expended in the Experiment No. 4, as in that No. 20; and hence we may conclude with the utmost certainty, that of the heat generated, or which, with proper management, might have been generated in the combustion of the Fuel used in the 4th Experiment, less than $\frac{1}{8}$ part was employed in heating the water;—the remainder, amounting to more than $\frac{7}{8}$ of the whole quantity, being dispersed and lost.

I ventured to give it as my opinion in the beginning of this Essay, that “not less than *seven-eighths* of the heat generated, or which, with proper management,

“nagement, might be generated from the Fuel actually consumed, is carried up into the atmosphere “with the smoke, and totally lost.”—I will leave it to my Reader to judge whether this opinion was not founded on good and sufficient grounds.

But though it be proved beyond the possibility of a doubt, that the process of heating water was performed in the 20th Experiment with about $\frac{1}{15}$ part of the proportion of Fuel which was actually expended in the 4th Experiment, yet neither of these Experiments, nor any deductions that can be founded on their results, can give us any light with respect to the *real* loss of heat, or how much less Fuel would be sufficient were there no loss whatever of heat. The Experiments show that the loss of heat must have been at least *eighteen times* greater in one case than in the other; but they do not afford grounds to form even a probable conjecture respecting the amount of the loss of heat in the Experiment in which the Economy of Fuel was carried the farthest, or the possibility of any farther improvements in the construction of fire-places. I shall, however, by availing myself of the labours of others, and comparing the results of their Experiments with mine, endeavour to throw some light on this abstruse subject.

Doctor CRAWFORD found, by an Experiment contrived with much ingenuity, and which appears to have been executed with the utmost care, that the heat generated in the combustion of 30 grains
of

of charcoal raised the temperature of 31 lbs. 7 oz. Troy = 181,920 grains, of water, $1\frac{7}{8}$ degrees of Fahrenheit's thermometer, *when none of the heat generated was suffered to escape.*

But if 30 grains of charcoal are necessary to raise the temperature of 181,920 grains of water $1\frac{7}{8}$ degrees, it would require 3157.9 grains of charcoal to raise the temperature of the same quantity of water 180 degrees, or from the point of freezing to that of boiling; for it is 1.71° to 30 grains, as 180° to 3157.9 grains. Consequently the heat generated in the combustion of 1 lb. of charcoal would be sufficient to heat 57.608 lbs. of ice-cold water 180 degrees, or to make it boil;—for 3157.9 grains of charcoal are to 181,920 grains of water as 1 lb. of charcoal to 57.608 lbs. of water.

From the results of Mr. LAVOISIER's Experiments, it appeared that the quantities of heat generated in the combustion of equal weights of charcoal and dry oak, are as 1089 to 600. Hence we may conclude, that equal quantities of heat are generated by 1 lb. of charcoal and 1.815 lbs. of oak; consequently that the heat generated in the combustion of 1.815 lbs. of oak, would heat 57.608 lbs. of ice-cold water,—or 1 lb. of oak, 31.74 lbs. of ice-cold water 180 degrees, or cause it to boil;—*were no part of the heat generated in the combustion of Fuel lost.*

If now we suppose the quantities of heat producible from equal weights of dry oak and of dry pine-

pine-wood to be equal,—(and there is reason to believe that this supposition cannot be far from the truth,) we can estimate the real loss of heat in each of the two Experiments before mentioned, (No. 4 and No. 20,) as also in every other case in which the quantity of Fuel consumed, and the effects produced by the heat, are known.

Thus, for instance, in the 20th Experiment, as the effects actually indicated that with *that part* of the heat generated in the combustion of 1 lb. of the Fuel which *entered the boiler*, $20\frac{1}{6}$ lbs. of ice-cold water might have been made to boil; as by the above estimate it appears that $31\frac{7}{8}$ lbs. of ice-cold water might be made to boil with *all* the heat generated in the combustion of 1 lb. of the Fuel—it is evident that about *one-third* of the heat generated was lost; or $\frac{20\frac{1}{6}}{31\frac{7}{8}}$ of it was saved.

This loss is certainly not greater than might reasonably have been expected, especially when we consider all the various causes which conspire in producing it; and I doubt whether the Economy of Fuel will ever be carried much farther.

In the Experiment No. 4, as the effects produced by the heat which entered the Boiler indicated that no more than 1.14 lb. of ice-cold water could have been made to boil with 1 lb. of the Fuel, it appears that in this Experiment only about $\frac{2}{3}$ th part of the heat generated was saved.

In all the Experiments made on a very large scale, with Brewhouse Boilers, rather more than *one-half* of the heat generated found its way up the chimney, and was lost.

C H A P. VI.

A short Account of a Number of Kitchens, public and private, and Fire-places for various Uses, which have been constructed under the Direction of the Author, in different Places.—Of the Kitchen of the HOUSE of INDUSTRY at MUNICH—Of that of the MILITARY ACADEMY—Of that of the MILITARY MESS-HOUSE—That of the FARM-HOUSE, and those belonging to the INN in the ENGLISH GARDEN at MUNICH.—Of the Kitchens of the Hospitals of LA PIETA and LA MISERICORDIA at VERONA.—Of a small Kitchen fitted up as a Model in the House of SIR JOHN SINCLAIR Bart. in LONDON.—Of the Kitchen of the FOUNDLING HOSPITAL in LONDON.—Of a MILITARY KITCHEN for the Use of TROOPS in CAMP.—Of a PORTABLE BOILER for the Use of TROOPS on a MARCH.—Of a large BOILER fitted up as a Model for BLEACHERS at the LINEN-HALL in DUBLIN.—Of a Fire-place for COOKING, and at the same Time WARMING A LARGE HALL; and of a PERPETUAL OVEN, both fitted up in the HOUSE of INDUSTRY at DUBLIN.—Of the KITCHEN—LAUNDRY—CHIMNEY FIRE-PLACES—COTTAGE FIRE-PLACE—and Model of

*of a LIME-KILN—fitted up in IRELAND in the
House of the DUBLIN SOCIETY.*

MY wish to give the most complete information possible with regard to the *grounds* on which the improvements I propose are founded, has induced me to be very particular in my account of my Experiments, and of the conclusions and practical inferences I have thought myself authorised to draw from them; and as these investigations have frequently led me into abstruse philosophical disquisitions which might not perhaps be very interesting to many of my readers, to whom a simple account of my Fire-places, with directions for constructing them, might be really useful; in order to accommodate readers of all descriptions, I have thought it best to divide my subject, and to reserve what I have still to say on the mechanical part of it,—the Construction of Kitchen Fire-places,—for a separate Essay. In the mean time, for the information of those who may have opportunities of examining any of the Kitchens, or Fire-places, for other purposes, which have already been constructed on my principles, under my direction, I have annexed the following account of them, and of the particular merits and imperfections of each of them. This account, added to what has been said in the foregoing Chapters of this Essay on the construction of Fire-places, will, I flatter myself, be found sufficient to convey the fullest information respecting the subject under consideration, and enable those,

those, who may wish to adopt the proposed improvements, to construct Fire-places of all kinds on the principles recommended, without any farther assistance.

Those who may not have leisure to enter into these scientific investigations, and who, notwithstanding, may wish to imitate these inventions, will find all the information they can want in my next Essay.

*An Account of the Kitchen of the HOUSE of INDUSTRY
at MUNICH, in its present State.*

THE large circular copper boiler (which is situated in a small room adjoining to the great Kitchen) is fitted up in a very complete manner;—its (wooden) cover is cheap, simple, and durable, and answers perfectly well for confining the heat;—the steam-tube (or steam-chimney, as I have called it) is very useful, as it carries off all the steam generated in cooking, and keeps the air of the Kitchen dry and wholesome.—To carry off the steam which rises from the hot soup when it is served up, there is a steam-chimney of wood, (furnished with a valve,) the opening of which is situated at the highest part of the Kitchen. To prevent the cold air from coming down by this passage into the Kitchen, its damper (which is opened and shut by a cord which goes over a pulley) is, in winter, kept constantly shut, except just when it

is necessary to open it for a moment to let out the steam.

The only alteration I would make, were I to fit up this boiler again, would be to leave openings by which the flues might be cleaned occasionally, without lifting the boiler out of its place.—This should be done in the fire-places of all large boilers. This boiler, which is used every day, requires to have its flues cleaned, and its bottom and sides scrubbed with a broom, to free them of soot, once in six weeks.

Over against this boiler is a machine for drying potatoes, which has been found to answer perfectly well the end for which it was contrived. Potatoes first moderately boiled, and then skinned and cut into thin slices, and dried in this machine, may be kept good for many years.

The eight iron boilers in the *great Kitchen* are fitted up on good principles; and the oven which is heated by the smoke from the fire-places of two of these boilers, which oven is destined for drying the wood for the use of this Kitchen, is deserving of attention.

The wooden covers of these eight boilers, and the horizontal tubes, constructed of wood wound round with canvas and painted with oil colours, by which the steam is carried off, have been found to answer very well the purposes for which they were contrived.

The Kitchen of the Military Academy at MUNICH.

This Kitchen in its present state is so perfect in all its parts, that I do not think it capable of any considerable improvement. The *roaster*, which has been in daily use *seven years*, is still in good condition, and bids fair to last *twenty years* longer.—It is large and roomy, and has been found to be extremely useful. Though the different parts of this Kitchen are not distributed with so much symmetry as could have been wished, owing to local circumstances, yet it is very complete in its various details, and all the various processes of cookery are performed in it with little labour, and with a very small expence indeed of Fuel. Two large boilers and three large saucepans, which are fitted up in a detached mass of brick-work in a corner of the room, (on the right hand on going into it,) I can recommend as perfect models for imitation. In short, I know of nothing which I could wish to alter in this Kitchen.—To say the truth, it has already undergone a sufficient number of changes and alterations.

The Kitchen in the Military Hall or Officers' Mess-House in the English Garden at MUNICH.

This Kitchen is much less perfect in its details than that just mentioned.—It was built in the spring of the year 1790, and has since undergone
 5 only

only a few trifling alterations. It has three roasters, which are made small on purpose to serve as models for private families; and I have had the pleasure to know that they have often been imitated.

The Kitchen in the Farm-House in the English Garden.

This Kitchen is well contrived for the use for which it was designed, and I can recommend it as a very good model for the Kitchens of Farm-houses, for families consisting of eighteen or twenty persons. One of the boilers, which is destined for warming water for the use of the Kitchen and the stables, is in winter heated by the smoke of a German stove which is situated in an adjoining room, —that inhabited by the Overseer of the farm.

The great Kitchen of the Inn in the Garden.

This Kitchen, which is adjoining to the Farm-house, is contrived almost for the sole purpose of roasting chickens before an open fire, a kind of food of which the Bavarians are extravagantly fond. It has three open fire-places, constructed on the principles recommended in my Essay on Chimney Fire-places, fronting different sides of the Kitchen, and all opening into the same chimney, which chimney is built nearly in the middle of the room. This Kitchen was built before my roasters were come into use,

The small Kitchen belonging to the Inn.

This Kitchen has nothing belonging to it which deserves attention, or which I would recommend for imitation. It was originally designed merely for making coffee, chocolate, &c.

A Kitchen which has lately been fitted up on my principles, in the new Hospital for the infirm and helpless Poor, which is situated on the height called the *Gasteig*, on the side of the river opposite to the town of Munich, is much more interesting, and is a good model for imitation.

The Kitchen of the Hospital of La Pieta at VERONA

Is peculiarly interesting, on account of its convenient form and the perfect symmetry of its parts.

The mass of brick-work in which the boilers are fixed occupies the middle of one side of a large high room, which is plaistered and white-washed, and neatly paved. The covers of the large boilers are lifted up by ropes which go over pulleys fixed to the ceiling of the top of the room; but were I to build the Kitchen again, I should substitute wooden covers with steam-chimnies instead of them, such in all respects as that belonging to the large round copper boiler in the Kitchen of the House of Industry at Munich. When the covers are so large that they cannot conveniently be lifted on and off with the hand, they should, in my opinion, always be

be made of wood, and divided into parts, united by hinges. When they are designed for confining the steam *entirely*, they should be made on a peculiar construction, which will hereafter be described. The covers for small boilers, and those for saucépans, should always be of tin, and double.

The grates on which the fires are made under the boilers in the Kitchen of the Hospital of *La Piéta*, are circular; but they are not hollow, or dishing, as that improvement did not occur to me till after that Kitchen was finished. The spiral flues under the boilers are also wanting, and for the same reason. In all other respects this Kitchen is, I believe, quite perfect.

The Kitchen of the Hospital of La Misericordia at
VERONA

Is constructed on the same principles as that of *La Piéta*. The only difference between them is in the distribution of the boilers. That of *La Misericordia* is built round two sides of the room. In many cases, this manner of disposing of the boilers will be found more convenient than any other; but in all cases where this method of placing them is preferred, care must be taken to place the largest boilers farthest from the chimney, and the smaller ones nearer to it, and in regular succession as their sizes diminish. This is necessary, in order that in the mass of brick-work in which the boilers are fixed, there may be room behind

the smaller boilers for the canals which carry off the smoke from the large ones into the chimney.

This circumstance was attended to in constructing the small Kitchen which I fitted up last spring in the house of Sir John Sinclair, Bart. President of the Board of Agriculture, Whitehall, London. This Kitchen (which was intended to serve as a model, and is open to the public view at all hours) is by no means as perfect as I wished it to be:— Having been built during my journey to Ireland, several mistakes were made by the workmen I employed, who, though they have great merit in their different lines of business, had not *then* had sufficient experience in constructing Kitchens on my principles, to be able to execute such a job in my absence without committing some faults. Those which were most essential I corrected; but my stay in England after my return from Ireland was too short; and my time too much taken up with other matters, to rebuild the Kitchen from the foundation; which I was very desirous of doing, and which, with the permission of the Proprietor, I shall certainly do when I come to England again. The greatest fault of the Kitchen is the want of dampers to the canals by which the smoke is carried off from the closed fire-places of the boilers and saucepans into the chimney. These dampers should never be omitted in any fire-place, however small. They are necessary even in fire-places for the smallest saucepans, and no large boiler should on any account be without one. Some Experiments

ments I have lately made (since my return to Bavaria) have shewed me how very necessary these dampers are ; and I consider it as my duty to the Public to lose no time in recommending the general use of them. The flattering attention which has been paid by the Public to the various improvements I have taken the liberty to propose, not only demands my warmest gratitude, but lays me under an indispensable obligation to exert myself to the utmost to deserve their esteem, and to merit the distinguished marks of their confidence with which on so many occasions I have been honoured.

But to return to the Kitchen in the house of Sir John Sinclair (the place where the meetings of the Board of Agriculture are held, and where of course there is a great concourse of ingenious men from all parts of the kingdom,—of men zealous for the progress of useful improvements). As the room is very small, it was not possible to do more in it than just to fit up a few small boilers and saucepans, and one middling-sized roaster, such as might serve for a small family ; which last is a machine so very useful that I cannot help flattering myself that it will soon come into general use. The saving of Fuel which it occasions is almost incredible, and the meat roasted in it is remarkably well-tasted and high-flavoured.

One of these roasters, on a large scale, was put up, under my direction, in the Kitchen of the Foundling Hospital in London ; and though I could not stay in England to see it finished, I have

had the satisfaction to learn, since my arrival at Munich, from my friend Mr. Bernard, (who is Treasurer to the Hospital,) that it has answered even beyond his expectations. He informs me, that when 112 lbs. of beef are roasted in it at once, the expence for Fuel amounts to no more than *four pence* sterling; and this when the coals are reckoned at an uncommonly high price, namely, at 1 s. 4 d. the bushel.

In the roaster belonging to the Kitchen of the Military Academy at Munich, I caused 100 lbs. Bavarian weight (equal to 123.84 lbs. Avoirdupois) of veal, in *six large pieces*, to be roasted at once, as an Experiment; the Fuel consumed was 33 lbs. Bavarian weight of dry pine-wood, (equal to 40.86 lbs. Avoirdupois,) which (at $4\frac{1}{2}$ florins the *clafter*, weighing 2967 lbs. Bavarian weight) cost 3 creutzers, or about *one penny* sterling.

This Experiment was made in the year 1792. Happening to mention the result of it in a large company in London, soon after my arrival there in the autumn of the year 1795, I had the mortification to perceive very plainly by the countenances of my hearers how dangerous it is to promulgate very extraordinary truths. I afterwards grew more cautious, and should not now have ventured to publish this account, had not the results of Experiments equally surprising, which have been made with the roaster in the Kitchen of the Foundling Hospital, been made known to the Public,

Not

Not only the roaster, but the boilers also which have been put up under my direction in the Kitchen of the *FOUNDLING HOSPITAL*, have been found to answer very well ; and I am informed that several other great Hospitals are about to imitate them. As I left London before the Kitchen of the Foundling Hospital was entirely finished, I do not know whether there are dampers to the canals by which the smoke goes off from the fire-places of the boilers, and from that of the roasters, to the chimney. If there are not, I could wish they might still be added ; and I would strongly recommend it to those who may be engaged in constructing Kitchen fire-places on my principles, never to omit them.

Oval graters of cast iron, in the form of a dish, such as I have described in the foregoing Chapters of this Essay, were tried in the Kitchen of the Foundling Hospital, but the heat was found to be so intense that they were soon melted and destroyed ; and we were obliged to have recourse to common flat grates, composed of strong bars of cast iron. Perhaps the heat generated in the combustion of pit-coal is so intense, when completely confined, (as it ought always to be in closed fire-places,) that it will not be possible, where coals are used as Fuel, to use the hollow dishing grates I have introduced in the public Kitchens at Munich, and which have been described and recommended in this Essay.

Since

Since my return to Bavaria, I have made several Experiments with grates composed of common bricks, placed edgewise, and I find that they answer for that use full as well, if not better, than iron bars. By making bricks *on purpose* for this use, of proper forms and dimensions, and composed of the best clay mixed with broken crucibles beaten to a coarse powder, Kitchen fire-places might be fitted up with them, which would be both cheap and durable, and as perfect in all other respects as any that could possibly be made, even were the most costly materials to be used in their construction.

To diminish still farther the expence attending the construction of closed Kitchen fire-places designed for the use of poor families, the opening by which Fuel is introduced might be closed with a brick, or with a flat stone;—another brick, or stone, might be made to serve at the same time as a register and a door to the ash-pit, and a third as a damper to the chimney or canal for carrying off the smoke from the fire-place.

I lately had an opportunity of fitting up a Kitchen on these principles, in the construction of which there was not a particle of iron used, or of any other metal, except for the boiler. On the approach of the French army under General Moreau in August last, the Bavarian troops being assembled at Munich (under my command) for the defence of the capital, the town was so full of soldiers,

diers, that several regiments were obliged to be quartered in public buildings, and encamped on the ramparts, where they had no conveniencies for cooking.—For the accommodation of a part of them, four large oblong square boilers, composed of very thin sheet coppers well tinned, were fitted up in a mass of brick-work in the form of a cross; each boiler with its separate fire-place, communicating by double canals, furnished with dampers, with one common chimney, which stands in the centre of the cross. The dampers are thin flat tiles;—the grates on which the Fuel is burned are composed of common bricks, placed edgewise;—and the passages leading to the fire-place, and to the ash-pit, are closed by bricks which are made to slide in grooves.

Under the bottom of each boiler, which is quite flat, there are three flues, in the direction of its length; that in the middle, which is as wide as both the others, being occupied by the burning Fuel. The opening by which the Fuel is introduced is at the end of the boiler *farthest from the chimney*; and the flame running along the middle flue to the end of it, divides there, and returning in the two side flues to the hither end of the boiler, there rises up into two other flues, in which it passes along the outside of the boiler into the chimney. The boilers are furnished with wooden covers divided into two equal parts, united by hinges. In order that the four boilers may be transported with greater facility from place to place, (from one camp to another

other for instance,) they are not all precisely of the same size, but one is so much less than the other, that they may be packed one in the other. The largest of them, which contains the three others, is packed in a wooden chest, which is made just large enough to receive it. In the smallest may be packed a circular tent, sufficiently large to cover them all. In the middle of the tent there must be a whole through which the chimney must pass. The four boilers, together with the tent, and all the apparatus and utensils necessary for a Kitchen on this construction for a regiment consisting of 1000 men, might easily be transported from place to place on an Irish car drawn by a single horse.

I have been the more particular in my account of this portable Kitchen, as I think it would be found very useful for troops in camp. The Right Honourable Mr. Thomas Pelham made a trial of one of them last summer for his regiment, (the Suffex militia,) and found it to be very useful. The saving of Fuel was very considerable indeed; and the saving of trouble in cooking not less important. The first Experiment we made together in a single boiler, fitted up for the purpose in the open air, in the middle of the court-yard of Lord Pelham's house in London.

I ought perhaps to have reserved what I have here said on the subject of these Military portable Kitchens for my next Essay, where it would more naturally have found its place; but being persuaded of the great advantages that may be derived

rived from them, I am unwilling to lose a moment in recommending them to the attention of those who have it in their power to bring them into use.

Those who wish to know more about them may, I am confident, procure every information they can desire respecting them, by applying to Mr. Pelham, or to any of the Officers of the Suffex militia who were in camp with the regiment last summer.

There is one more invention for the use of armies in the field, which I wish to recommend, and that is a *portable boiler* of a light and cheap construction, in which victuals may be cooked *on a march*. There are so many occasions when it would be very desirable to be able to give soldiers, harassed and fatigued with severe service, a warm meal, when it is impossible to stop to light fires and boil the pot, that I cannot help flattering myself that a contrivance, by which the pot *actually boiling* may be made to keep pace with the troops as they advance, will be an acceptable present to every humane officer and wise and prudent general. Many a battle has undoubtedly been lost for the want of a good comfortable meal of warm victuals to recruit the strength and raise the spirits of troops fainting with hunger and excessive fatigue.

But to return from this digression.—The form of the two principal boilers in the Kitchen of the FOUNDLING HOSPITAL is that of an oblong square; that form which on several accounts I have reason to think preferable to all others for large
boilers,

boilers, but especially on account of the facility of fitting them up with square bricks, and of cleaning their flues, I first introduced in Ireland in several fire-places designed for different uses, which I fitted up as models, in Dublin, during the visit I made last spring to that country on the invitation of my friend Mr. Secretary Pelham.

The first of these oblong square boilers is that which is fitted up in the court-yard of the Linenhall at Dublin, *as a model for bleachers*: It is 8 feet wide, 10 feet long, and 2 feet deep; and it is furnished with a wooden cover, which shutting down in a groove in which there is a small quantity of water, the steam is by these means confined in the boiler. This cover is moveable on its hinges, which are placed at the end of the boiler farthest from the door of the fire-place; and it is occasionally lifted up by means of a rope, which goes over a compound pulley which is fixed over the boiler at the top or ceiling of the room.

Under this boiler there are five flues which run in the direction of its length, and are arranged and constructed in the same manner as the flues of the new Brewhouse boiler which I lately fitted up at Munich.—(See Fig. 21, Plate V.)—There are no flues round the outside of this boiler; but the brick walls by which they are defended from the cold air are double, and the space between them is filled with charcoal dust.

The Fuel burns at the hither end of the middle flue, in an oval dish-grate; and the flame running
along

along in this flue under the middle of the boiler to the farther end of it, there divides, and returns in the two adjoining flues:—It then turns to the right and left, and going back again in the two outside flues to the farther end of the boiler, goes out from under it, there, in two canals, which sloping upwards conduct it to the flues of a *second boiler* of equal dimensions with the first, where it circulates, and warms the water which is designed for refilling the first boiler.

As these boilers are made of exceedingly thin sheet copper, and *thin boilers* are stronger to resist the effects of the Fire, and consequently more durable, than very thick ones, they both together cost much less than one single boiler on the common construction; and Mr. Duffin, Secretary to the Linen-board, who is a very active, intelligent man, and is himself engaged in a large concern in the bleaching business, showed me a computation founded on actual Experiments which he himself made with this new boiler, by which he proved that the saving of Fuel which will result from the general introduction of these boilers in the bleaching trade throughout Ireland will amount to at least fifty thousand pounds sterling a year.

In a LAUNDRY which I fitted up in the house belonging to the Dublin Society, (and which is designed to serve as a model for Laundries for private gentlemen's families,) there are also two oblong square boilers, the one heated by the Fire, and the other by the smoke; and this smoke, after having
circulated

circulated in the flues under the second boiler, passes through a long flue (constructed like hot-house flues) which goes round two sides of the *drying-room*, (which is adjoining to the *washing-room*,) and then passing through the wall of the drying-room into the ironing-room, it goes off into an open chimney. As the bottom of the second boiler lies on a level with the top of the first, the warm water runs out of the second to refill the first, by a tube furnished with a brass cock, which greatly facilitates the filling of the principal boiler. The wooden covers of these boilers, which are double and moveable on hinges, are shut down in grooves in which there is water, and the steam being by these means confined, is forced to pass off by a wooden tube standing on a part of the cover which is fastened down to the boiler with hooks, carries the steam upwards to the height of seven or eight feet, where it goes off laterally by another (horizontal) wooden tube, through the wall into the drying-room. As soon as this horizontal wooden tube has passed through the wall into the drying-room, it ends in a copper tube, about 3 inches in diameter, which, lying nearly in a horizontal position, conducts the steam through the middle of the drying-room in the direction of its length, and through a hole in a window at the end of the room into the open air.

The steam in passing through the drying-room in a metallic tube, (which is a good conductor of heat,) gives off its heat through the sides of the

tube to the air of the room, and the water which is condensed runs off through the tube. By sloping the tube *upwards*, instead of downwards, as by accident it was sloped, the condensed water, which is always nearly boiling hot, when it is condensed, might be made to return into the boiler, which would be attended with a saving of heat, and consequently of Fuel.

The furnace for heating the irons used in smoothing the linen, (or ironing, as it is called,) is a kind of oven built of bricks and mortar, the bottom of which is a shallow pan of cast iron, 18 inches square and about 3 inches deep, which is nearly filled with fine sand. The Fire being made under this pan in a closed fire-place, as the sand defends the upper surface of the pan from the cold air of the atmosphere, the pan is commonly red-hot, and the irons, being shoved down through the sand and placed in contact with this plate of red-hot metal, are heated in a very short time, and at a small expence of Fuel.

This contrivance might be used with great success for covering the *hot plates* on which sauce-pans are made to boil in many private Kitchens.

This stove, or oven, for heating the smoothing-irons, projects into the drying-room, but the door by which the irons are introduced, as well as that leading to the fire-place, and that leading to the ash-pit, all open into the ironing-room.

The smoke goes off through the drying-room in an iron tube, and assists in warming the room and in drying the linen.

As it may sometimes be necessary to heat the drying-room when neither the wash-house boilers nor the stove for heating the smoothing-irons are heated, provision is made for that, by constructing a small closed fire-place, designed merely for that purpose, which opens into the flue, by which the smoke from the boilers is carried round the drying-room. This fire-place (which is never used but when it is wanted for drying the linen) is situated just without the drying-room, under the end of the flue where it joins the second boiler.— The opening at the top of its fire-place, by which the flame of the burning Fuel enters the under part of the flue, is kept closed by a sliding plate of iron, or damper, when this fire-place is not used; and when it is used, the door which closes the opening into the fire-place of the first or principal boiler, and the register in its ash-pit door, are kept shut.

That the top of the principal boiler might not be too high above the pavement of the wash-house for the laundresses to work in the boiler without being obliged to go up steps or stairs, the grate and the bottom of the flues under the boiler are nearly on a level with the pavement, and the ash-pit is sunk into the ground; and to render the approach to the opening into the fire-place more convenient in introducing the Fuel and lighting and managing the Fire, there is an area before the fire-place; about 3 feet square and 2 feet deep, sunk in the ground, and walled up on its sides, into which there is a descent by steps. In two of the sides of these

these vertical walls (those on the right and left when you stand fronting the fire-place) there are vaults, for containing Fuel, which extend several feet under the pavement. The steps which descend into this area are on the side of it, opposite the fire-place.

Areas of this kind are very necessary for all fire-places for large boilers, otherwise the top of the boiler will necessarily be raised too high above the level of the pavement to be approached with facility and convenience. Steps may be made, it is true, for approaching boilers which are placed higher; but these are always inconvenient, and take up more room, and cost more than the execution of the plan here proposed for rendering them unnecessary.

The areas before the fire-place door of the large boilers in the Kitchen of the Foundling Hospital are occasionally closed by trap-doors. As often as this is done there must be a number of small holes bored in the door to permit the air necessary for feeding the Fire to descend into the ash-pit; and when the bottom of the passage leading into the fire-place happens to lie above the level of the upper surface of this trap-door, the part of the door immediately under this opening, should, to prevent accidents from live coals which may occasionally fall out of the fire-place, be covered with a thin plate of sheet-iron.

When large boilers are fitted up in situations where it is not possible to sink an area in front of

the fire-place, the mass of brick-work in which the boiler is set must be raised, and steps must be made to approach it. When this is done, the upper step should be made very wide, (at least two feet,) in order that there may be room to stand and work in the boiler; and for still greater convenience, the steps should be continued round three sides of the boiler, when the boiler stands in a detached mass of brick-work. The bottom of the door of the fire-place, should, if possible, be above the upper flat surface of the upper step; and to preserve the symmetry of the whole, the ash-pit door may be in the front of the upper step, and the passage into the ash-pit, (which will be long of course) may descend in a gentle slope. In this manner the Kitchen of the Hospital of *la Pieta* at *Verona* was constructed.

No inconvenience whatever attends the increase of the length of the passage into the ash-pit, except it be that very trifling one,—(which surely does not deserve to be mentioned,)—the increase of labour attending the removal of the ashes; but the inconvenience would be very considerable which would unavoidably attend the discontinuation, or breaking off, of the steps round the hither end, or front of the boiler, which would be necessary in order to be able to place the ash-pit door *directly under* the fire-place door, and to make a way to approach it.

The flues under the principal boiler of the Laundry in the House of the Dublin Society, are not contrived so as to divide the flame and cause it to circulate

culate in *two* currents ; they run from side to side under it : the door of the fire-place is not in the middle, but on one side of the boiler, and near one end of it. The flame passing and returning (under the boiler) twice from its front to its opposite side, goes off at its end (that farthest from the fire-place) into a canal furnished with a damper, which canal, rising upward at an angle of about 45 degrees, leads to the flues under the second boiler. The bottom of the flues of the principal boiler are just on a level with the pavement of the wash-house ; and, in order that they may easily be cleaned out, and the bottom of the boiler scrubbed with a broom to free it from soot, the ends of the flues are, in building the fire-place, left open, and afterwards, when the boiler is set, they are closed by temporary (double) walls of dry bricks. To make these walls tight, the joinings of the bricks are plastered on the outside with moist clay.

The sides of the boilers are defended from the cold air by thin walls of bricks covered with wainscot, and by filling the space between these walls and the boiler with pounded charcoal. Were I to fit up these boilers again, I should leave this space void, or filled merely with air, forming several small openings below, through which the flame and hot vapour from the flues might ascend and surround the boiler. In the large boiler fitted up in the Linen-hall as a model for bleachers, this alteration is also necessary to render it complete ; and as it might be made in a few hours, and almost with-

out any expence, I cannot help expressing a wish that it might still be done.

The ardent zeal for the prosperity of his country, and indefatigable attention to every thing that tends to promote useful improvement, which so eminently distinguish that enlightened patriot and most respectable statesman, to whom the manufactures and commerce of Ireland, and the linen-trade in particular, are so much indebted, encourage me to hope that he will take pleasure in giving his assistance to render the models for improving fire-places and saving Fuel, which I have had the satisfaction of leaving in Ireland, as free from faults as they can possibly be made.

Though my stay in Ireland was too short to construct models of all the improvements I wished to have introduced in that delightful and most interesting island, yet the liberality with which my various proposals were received, and the generous assistance I met with from all quarters, enabled me to do more in two months than I probably should have been able to have effected in as many years in some other older countries, where the progress of wealth and of refinements have rendered it extremely difficult to get people to attend to useful improvements.

I wished much to have been able to have fitted up the great Kitchen in the House of Industry at Dublin, as the expence of Fuel is very considerable in that extensive establishment, where more than 1500 persons are fed daily, at an average; but not
having

having time to finish so considerable an undertaking, I thought it most prudent not to begin it. I fitted up one large boiler as a model at one end of one of the working-halls; but this was designed principally to show how a large hall might be heated from a Kitchen fire-place, and from the very same Fire which is used for cooking*. The smoke from the fire-place is carried along horizontally on one side of the hall from one end of it to the other; and the boiler being closed by a cover which is steam-tight, the steam from the boiler is also forced along from one end of the hall to the other, in an horizontal leaden pipe, which runs parallel to the flue occupied by the smoke, and lies immediately over it. In warm weather, when the hall does not require to be heated, the smoke and steam go off immediately into the atmosphere by a chimney adjoining to the fire-place, without passing through the hall.

To be able to equalize the heat in the hall, (which is very long and narrow,)—or to render it as warm at the end of it which is farthest from the fire-place, as at that next the Fire, I directed clothing for the steam tube of warm blanketing to be made in lengths of three or four feet, to be occasionally put round it and fastened by buttons.

By clothing or covering the steam tube more or less, as may be found necessary in those parts of

* This contrivance might easily be applied to the heating of hot-houses, even though the hot-house should happen to be situated at a considerable distance from the Kitchen.

the hall where the heat is greatest, the steam being by this covering prevented from giving off its heat to the air through the tube, will go on farther and warm those parts of the hall which otherwise would be not sufficiently heated. The steam tube, which is constructed of very thin sheet-lead, is about 3 inches in diameter, and instead of being laid exactly in a horizontal position, slopes a little upwards, just so much that the water which results from the condensation of the steam may return into the boiler*.

The horizontal flue through which the smoke passes is a round tube of sheet-iron, about 7 inches in diameter, divided, for the facility of cleaning it, in lengths of 12 or 15 feet, fixed nearly horizontally at different heights from the floor, or, in an interrupted line, in hollow pilasters or square columns of brick-work. A common hot-house flue, constructed of bricks and mortar, would have an-

* I contrived a fire-place for heating one of the principal churches in Dublin on these principles with steam (but without making use of the smoke); and I promised to give a plan (which, I am ashamed to say, I have not yet been able to finish) for heating the superb new building destined for the meeting of the Irish House of Commons.

One of the two chimney fire-places, which I fitted up in the hall in which the meetings of the Royal Irish Academy are held, will, I imagine, be found to answer very well for heating high rooms and large halls in private houses. In this fire-place I have endeavoured, and I believe successfully, to unite the advantages of an open fire with those of a German stove. The grate used in fitting up this fire-place, and which is of cast iron, and far from being unelegant in its form, and which cost only *seven shillings and sixpence sterling*, is decidedly the best adapted for open chimney fire-places, where coals are used as fuel, of any I have yet seen. By a letter I lately received from a friend in Ireland, I had the satisfaction to learn that these grates are coming very fast into general use in that country.

swered equally well for warming the hall, but would have taken up too much room, which is the only reason it was not preferred to these iron tubes.

In constructing the boiler, (which is of thin sheet-iron,) I made an Experiment which succeeded even beyond my expectation. The flues under the boiler—(and there are none round it)—are projections from the bottom of the boiler;—they are hollow walls of sheet-iron, about 9 inches high, and an inch and three quarters thick, into which the liquid in the boiler descends, and which in fact constitute a part of the boiler. By this contrivance the flame is surrounded on all sides, except at the bottom of the flues, (where the heat has little or no tendency to pass,) by the liquid which is heated, and the fire-place is merely a flat mass of brick-work. The grate is even with the upper surface of this mass of brick-work, and the ash-pit is the only cavity in it.

In constructing the boiler, provision was made, by omitting or interrupting the hollow walls or divisions of the flues, in the proper places, to leave room for introducing the Fuel,—for the passage of the flame from one flue to another,—and from the last flue into the canal by which the smoke goes off into the chimney, or into the iron tubes by which the hall is occasionally warmed.

One principal object which I had in view in this Experiment was to see if I could not contrive a boiler, which, being suspended under a waggon or other wheel carriage, might serve for cooking for

troops on a march; or which, being merely set down on the ground, a fire might be immediately kindled under it.

Those who will take the trouble to examine the boiler in question, will find that the principle on which it is constructed may easily be applied to the objects here mentioned. But it is not merely for portable boilers that this construction would be found useful; I am convinced that it would be very advantageous for the boilers of steam-engines,—for distilleries,—and for various other purposes. As the escape of heat into the brick-work is almost entirely prevented, and as the surface of the boiler on which the heat is made to act is greatly increased by means of the hollow walls, the liquid in the boiler is heated in a very short time, and with a small quantity of Fuel.

There is still another advantage attending this construction, which renders it highly deserving the attention of distillers. By making the tops of the flues arched instead of flat, (which may easily be done, and which is actually done in the boiler in question,) or in the form of the roof of a house, as the hottest part of the flame will of course always occupy the upper part of the flues, and as the thick or viscous part of the liquor in the boiler, that which is in most danger of being burned to the bottom of the boiler, and giving a bad taste to the spirit which comes over, cannot well lie on the convex or sloping surface of these flues, there will be

be less danger of an accident which distillers have hitherto found it extremely difficult to prevent.

In constructing boilers on these principles for distillers, it will probably be found necessary to increase very much the thickness of the hollow walls of the flues; and perhaps to make them even deeper than the level of the bottom of the flues, in order more effectually to prevent the thick matter which will naturally settle in those cavities from being exposed to too great a heat.

A similar advantage will attend large boilers constructed on these principles for making thick soups for hospitals; these soups being very apt to burn to the bottoms of the boilers in which they are prepared.

I made another Experiment in the House of Industry in Dublin, which I wished much to have had time to have prosecuted farther. Finding that the expence for wheaten bread for the House was very great, (amounting, in the year 1795, to no less than 3841 l. sterling,) I saw that a very considerable saving might be made by furnishing those who were fed at the public expence with oaten cakes, (a kind of bread to which they had always been used,) instead of rendering them dainty and spoiling them by giving them the best wheaten bread that could be procured, as I found had hitherto been done. But to be able to furnish oaten cakes in sufficient quantities to feed 1500 persons, some more convenient method of baking them than that commonly practised was necessary, and one in which

which the expence of Fuel might be greatly lessened.

With a view to facilitate this important change in the mode of feeding the numerous objects of charity, and of *correction*, who were shut up together within the walls of that extensive establishment, I constructed what I would call a *Perpetual Oven*.

In the centre of a circular, or rather cylindrical mass of brick-work, about eight feet in diameter, which occupies the middle of a large room on the ground floor, I constructed a small circular closed fire-place for burning either wood, peat, turf, or coals. The diameter of the fire-place is about eleven inches, the grate being placed about ten inches above the floor, and the top of the fire-place is contracted to about four inches. Immediately above this narrow throat, six separate canals (each furnished with a damper, by means of which its opening can be contracted more or less, or entirely closed) go off horizontally, by which the flame is conducted into six separate sets of flues, under six large plates of cast iron, which form the bottoms of six Ovens on the same level, and joining each other by their sides, which are concealed in the cylindrical mass of the brick-work. Each of these plates of cast iron being in the form of an equilateral triangle, they all unite in the centre of the cylindrical mass of brick-work, consequently the two sides of each unite in a point at the bottom of it, forming an angle of sixty degrees.

The

The flame, after circulating under the bottoms of these Ovens, rises up in two canals concealed in the front wall of each Oven, and situated on the right and left of its mouth, and after circulating again in similar flues on the upper flat surface of another triangular plate of cast iron which forms the top of the Oven, goes off upwards by a canal furnished with a damper into a hollow place, situated on the top of the cylindrical mass of the brick-work, from which it passes off in a horizontal iron tube, about seven inches in diameter, suspended near the ceiling of the room, into a chimney situated on one side of the room.

These six Ovens, which are contiguous to each other in this mass of brick-work, are united by their sides by thin walls made of tiles about $1\frac{1}{2}$ inches thick, and 10 inches square, placed edgewise, and each Oven having its separate canal, furnished with a register communicating with the fire-place, any one or more of them may be heated without heating the others, or the heat may be turned off from one of them to the other in continual succession; and, by managing matters properly, the process of baking may be *uninterrupted*. As soon as the bread is drawn out of one of the Ovens, the Fire may immediately be turned under it to heat it again, while that from under which the Fire is taken is filled with unbaked loaves, and closed up.

A principal object which I had in view in constructing this Oven was to prevent the great loss of heat

heat which is occasioned in large Ovens, by keeping the mouth of the Oven open for so considerable a length of time as is necessary for putting in and drawing out the bread. As one of these small Ovens contains only five large loaves, or cakes, it may be charged, or the bread when baked may be drawn, in a moment; and, during this time, the other five Ovens are kept closed, and consequently are not losing heat; *one* of them is heating, while the other *four* are filled with bread in different stages of the process of baking.

When I constructed this Oven, though I had no doubt of its being perfectly well calculated for the use for which it was principally designed,—baking oaten cakes, which are commonly baked on heated iron plates,—yet I was by no means sure it would answer for baking common bread in large thick loaves. I had not made the Experiment. And though I could not conceive that any thing more could be necessary in the process of baking than *heat*,—and here I was absolutely master of every degree of it that could possibly be wanted, and could even regulate the succession of different degrees of it at pleasure,—I thought it probable that some particular management might be required in baking bread in these metallic ovens, a knowledge of which could only be acquired by experience.

What served to strengthen these suspicions was a discovery which had accidentally been made by the cook of the Military Academy. In the course of *his* experiments, he found that my Roaster is admirably

mirably well calculated for baking pies, puddings, and pastry of all kinds;—provided however that the Fire be managed in a certain way;—for when the Fire is managed in the same manner in which it ought to be managed in roasting meat, pies and pastry will absolutely be spoiled. After repeated failures and disappointments, and after having lost all hopes of ever being able to succeed in his attempts, the cook (by mere accident, as he assured me) discovered the important secret;—and important he certainly considers it to be, and feels no small degree of satisfaction,—not to say pride,—in having been so fortunate as to make the discovery. He must pardon me if I take the liberty, —even without his permission,—to publish it to the world for the good of mankind.

The Roaster must be well heated before the pies or pastry are put into it, and the blowers must never be quite closed, during the process.

I have lately found that by using similar precautions, bread may be perfectly well baked in metallic Ovens, similar to that in the House of Industry in Dublin.

Thinking it more than probable that means might be devised for managing the heat in such a manner as to perform that process in Ovens constructed on these principles, and heated *from without*; and conceiving that not only a great saving of Fuel, but also several other very important advantages, could not fail to be derived from that discovery, on my return to Munich from England,
in

in August last, I immediately set about making Experiments, with a view to the investigation of that subject; and I have so far succeeded in them, that for these last four months my table has been supplied entirely with bread baked in my own house, by my cook, in an Oven constructed of thin sheet-iron, which is heated (like my Roasters) from without;—and I will venture to add, that I never tasted better bread. All those who have eaten of it have unanimously expressed the same opinion of it. It is very light,—most thoroughly baked without being too much dried,—and I think remarkably well-tasted. The loaves, which are made small in order that they may have a greater proportion of crust, (which, when the bread is baked in this way, is singularly delicate,) are placed in the Oven on circular plates of thin sheet-iron, raised about an inch on slender iron feet. Were the loaf placed on the bottom of the Oven, the under crust would presently be burnt to a coal, and the bread spoiled. A precaution absolutely necessary in baking bread in the manner here recommended, is to leave a passage for the steam generated in the process of baking to escape. This may be done either by constructing a steam-chimney for that purpose, furnished with a damper; or simply by making a register in the door of the Oven.

As this is not the proper place to enlarge on this subject, I shall leave it for the present; but I cannot help expressing a wish, that what I have here advanced may induce others, especially *Bakers*,
who

who may find their own advantage in the prosecution of these interesting and important investigations, to turn their attention to them.

How exceedingly useful would my Roasters be, and Ovens constructed on the principles here recommended, on shipboard!—Having served a campaign (as a volunteer) in a large fleet, (that commanded by Admiral Sir Charles Hardy in the year 1779,) and having made several long sea voyages, I have had frequent opportunities of seeing how difficult it is in bad weather to cook at sea; and it is easy to imagine how much it would contribute to the comfort of seafaring people, especially at times when they are exposed to the greatest fatigues and hardships, to enable them to have their tables well supplied with warm victuals.

In order that the motion of the vessel might not derange any part of the apparatus used in the process of cooking at sea in my Roasters, the form of the Roaster should be that of a perfect cylinder; and the dripping-pan in which the meat is placed should be a longitudinal section of another cylinder, less in diameter than the Roaster by about an inch, and suspended on two pivots in the axis of the Roaster, in such a manner that the dripping-pan may swing freely in the Roaster, without touching its sides. The Roaster should be placed in the brick-work, with its axis in the direction of the length of the ship; and to prevent the gravy from being thrown out of the dripping-pan when the vessel pitches, its hollow cavity should be divided

into a number of compartments, by partitions running across it from side to side.

It remains for me to give some account of the Kitchen which I fitted up in the House of the Dublin Society, as a model for private families ; and also of a Cottage Fire-place, and a Lime-kiln, which I constructed, as models for imitation, in the courtyard of that public building.

With regard to the Kitchen, it is necessary that I should remark at setting out, that it was not intended so much to serve as a model complete of a convenient Kitchen for a private family, as to display a variety of useful inventions, all or any of which may at pleasure be easily adopted, in Kitchens of all kinds and of all dimensions. I thought this would be more useful than any simple model of a Kitchen I could contrive.

It is however a very complete Kitchen ; and though there are some contrivances belonging to it which might have been omitted, yet they will all, I am confident, be found useful for the different purposes for which they were particularly designed ; and, in a Kitchen for a large family, would often come into use.

The general disposition of the various parts of this Kitchen I consider as being quite perfect. It is the same as that of the Hospital of *la Pieta* at *Verona* ; and of a very complete private Kitchen which was built about two years ago at *Monich*, under my direction, in the house of *BARON LERCHENFELD*, Steward of the Household to His
Most

MOST SERENE HIGHNESS THE ELECTOR. In my next Essay, which will treat exclusively of the Construction of Kitchen Fire-places, and of Kitchen Utensils, I shall give a particular detailed account of the manner in which the various Boilers,—Steam-boilers,—Saucepans,—Oven,—Roasters, &c.—are disposed and connected in the mass of brick-work in these Kitchens; and shall accompany these descriptions with a sufficient number of Plates to render them perfectly intelligible.

Cottage Fire-place, and Iron Pot, for cooking for the Poor.

THE Cottage Fire-place which I fitted up as a model, in the court-yard of the House of the Dublin Society, was not quite finished when I left Ireland; but an idea may be formed from what was done of the general principles on which such Fire-places may be constructed. On each side of the open chimney Fire-place, (which being small, was built in the middle of one much larger, which was constructed to represent a large open Fire-place, such as are now general in Cottages,) I fitted up an Iron Pot on a peculiar construction, cast by Mr. Jackson of Dublin, and designed for the use of a poor family in cooking their victuals. This Pot is nearly of a cylindrical form, about sixteen inches in diameter, and eight inches deep; and under its bottom, which is quite flat, there is a thin spiral projection, which was cast with the Pot, and serves instead of feet to it,

it, the turns of which, when the Pot is set down on a flat surface, form a spiral flue in which the flame circulates under the bottom of the Pot. This projection, which is near half an inch thick where it is united with the bottom of the Pot, and less than a quarter of an inch below where its lower edge rests on the ground, is about four inches wide, or rather deep. This projection was made tapering, in order to its being more easily cast. To defend the outside of this Pot from the cold air, the Pot is inclosed in a cylinder of thin sheet-iron, equal in diameter to the extreme width of the Pot at its brim,—just as high as the depth of the Pot and of its spiral flues taken together. The Pot is fastened to this cylindrical case by being driven into it with force, a rim in the form of a flat hoop, about an inch and an half deep and a little tapering, being cast on the outside of the Pot at its brim, the external surface of which was fitted exactly into the top of this cylinder. This projection is useful, not only in uniting the Pot to its cylindrical case, but also to keep this cylindrical case at some small distance from the sides of the Pot, by which means the heat is more effectually confined.

To be able to move about this Pot from place to place, it has two handles which are rivetted to the outside of its cylindrical case; and it is provided with a wooden cover.

I am sensible that I often expose myself to criticism by anticipating what would more naturally find its place elsewhere. But what I have here

said

said in regard to this Iron Pot is intended merely as hints to awaken the curiosity and excite the attention of ingenious men,—of such as take pleasure in exercising their ingenuity in contriving and perfecting useful inventions, and who delight in contemplating the progress of human industry.

Model of a perpetual Lime-kiln.

THE particular objects principally had in view in the construction of this Lime-kiln (which stands in the court-yard of the Dublin Society) were, *first*, to cause the Fuel to burn in such a manner as to consume the smoke, which was done by obliging the smoke to descend and pass through the Fire, in order that as much heat as possible might be generated.—Secondly, to cause the flame and hot vapour which rise from the Fire to come into contact with the lime-stone by a very large surface, in order to economise the heat, and prevent its going off into the atmosphere; which was done by making the body of the Kiln in the form of a hollow truncated cone, and very high in proportion to its diameter; and by filling it quite up to the top with lime-stone, the Fire being made to enter near the bottom of the cone.

Thirdly, to make the process of burning lime *perpetual*, in order to prevent the waste of heat which unavoidably attends the cooling of the Kiln in emptying and filling it, when, to perform that operation, it is necessary to put out the Fire.

And, fourthly, to contrive matters so that the lime in which the process of burning is *just finished*, and which of course is still *intensely hot*, may, in *cooling*, be made to give off its heat in such a manner as to assist in heating the fresh quantity of cold lime-stone with which the Kiln is replenished as often as a portion of lime is taken out of it.

To effectuate these purposes, the Fuel is not mixed with the lime-stone, but is burned in a closed fire-place, which opens into one side of the Kiln, some distance above the bottom of it. For large Lime-kilns on these principles there may be several fire-places, all opening into the same cone, and situated on different sides of it; which fire-places may be constructed and regulated like the fire-places of the furnaces used for burning porcelain.

At the bottom of the Kiln there is a door, which is occasionally opened to take out the lime.

When, in consequence of a portion of lime being drawn out of the Kiln, its contents settle down or subside, the empty space in the upper part of the Kiln, which is occasioned by this subtraction of the burned lime, is immediately filled up with fresh lime-stone.

As soon as a portion of lime is taken away, the door by which it is removed must be immediately shut, and the joinings well closed with moist clay, to prevent a draught of cold air through the Kiln. A small opening however must be left, for reasons which I shall presently explain.

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As the Fire enters the Kiln at some distance from the bottom of it, and as the flame *rises* as soon as it comes into this cavity, the lower part of the Kiln (that below the level of the bottom of the fire-place) is occupied by lime already burned, and as this lime is intensely hot, when, on a portion of lime from below being removed, it descends into this part of the Kiln, and as the air in the Kiln, to which it communicates its heat, must *rise upwards* in consequence of its being heated, and pass off through the top of the Kiln, this lime in cooling is, by this contrivance, made to assist in heating the fresh portion of cold lime-stone with which the Kiln is charged. To facilitate this communication of heat from the red-hot lime just burned to the lime-stone above in the upper part of the Kiln, a gentle draught of air through the Kiln from the bottom to the top of it must be established by leaving an opening in the door below, by which the cold air from without may be suffered to enter the Kiln. This opening (which should be furnished with some kind of a register) must be very small, otherwise it will occasion too strong a draught of cold air into the Kiln, and do more harm than good; and it will probably be found to be best to close it entirely, after the lime in the lower part of the Kiln has parted with a certain proportion of its heat.

Conceiving the improvement of Lime-kilns to be a matter of very great national importance, especially since the use of lime as manure has become so general, I intend to devote the first leisure time

I can spare to a thorough investigation of that subject;—in the mean time, I have here thrown out the loose ideas I have formed respecting it, in order that they may be examined, corrected, and improved upon by others who may be engaged in the same pursuits.

The model I caused to be constructed in the court-yard of the Dublin Society, is, I am sensible, very imperfect. It was built in a great hurry, being begun and finished the same day,—the day but one before I left Ireland;—but I am now engaged in constructing a Lime-kiln on the same principles, (for the use of the farm in the English Garden at Munich,) which I shall take pains to make as perfect as possible; and should it be found to answer as well as I have reason to hope it will, I shall not fail to give a particular account of it to the Public, accompanied with drawings, and all the details which shall be necessary in order to give the most satisfactory account of the result of the Experiment.

These investigations will be the more interesting, and their results more generally useful, as the discovery of a mine of pit-coal in the neighbourhood of Munich, which is now worked with success, has put it in my power to use coal as Fuel, as well as wood and turf, in the Experiments I shall make in burning lime in this Kiln.

For the information of those who may be disposed to engage in these pursuits, I have published the annexed sketch of the Lime-kiln in question, which

which is now actually building (see Plate VI.). I thought it right to do this, that we might start fair; and I can assure my competitors in this race, that I shall feel no ill will on seeing them get before me.

If I do not deceive myself, the laudable exertions of others afford me almost as much pleasure as my own pursuits;—at least I am quite certain that when I can flatter myself that I have had any,—even the smallest share,—in *exciting* those exertions, the satisfaction I feel in contemplating them is inexpressible.

DESCRIP.

which is now actually building (see Plate VI.) I thought it right to do this, that we might start fair; and I can assure my competitors in this race, that I shall feel no ill will on seeing them get before me.

If I do not deceive myself, the laudable exertions of others afford me almost as much pleasure as my own pursuits;—at least I can come certain that when I can flatter myself that I have had any—even the smallest share—in various good exertions, the satisfaction I feel in contemplating them is inexpressible.

It is a great pleasure to me to see the progress of the arts, and to be enabled to contribute to it in any way. I have been particularly interested in the progress of the steam engine, and in the various improvements which have been made in it. I have been particularly interested in the progress of the steam engine, and in the various improvements which have been made in it. I have been particularly interested in the progress of the steam engine, and in the various improvements which have been made in it.

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DESCRIPTION OF THE PLATES.

PLATE I.

FIG. 1. A view of a double cover for a boiler or faucepan. In this design the rim is seen which enters the boiler, and the tube by which the steam goes off is seen in part (above),—and is in part indicated by dotted lines. (See page 18.)

Fig. 2 shows this cover placed on its boiler. Part of the side of the cover is represented as wanting, in order that the steam tube might be better seen. The height of this cover is represented as being equal to *one-half* its diameter; but I have found *one-third* of its diameter quite sufficient for its height.

Fig. 3 and **Fig. 4** are views of my circular dishing-grates for closed kitchen fire-places. They may be made of any size, from 5 inches to 18 inches in diameter, according to the size of the boiler. The rules I have in general followed, in determining the size proper for the grate for any (circular) boiler, has been to make its diameter equal to half the diameter of the boiler at the brim. (See page 41.)

Fig. 5 is an inverted hollow cone of thin sheet-iron, which is placed immediately under the grate,

its brim being made to receive the circular rim of the grate. When the fire-place is large, this inverted cone may be made of fire-stone, or constructed of bricks and mortar. For small fire-places it may be made of earthen ware, which is perhaps the very best material for it that can be found. (See page 43.)

Fig. 6, Fig. 7, and Fig. 8, are views and sections of a perforated tile, with its stopper, such as are used for closing the entrance by which the Fuel is introduced into closed kitchen fire-places. The diameter of the circular opening, or hole in the tile, may be from 6 to 7 inches. (See page 30.)

PLATE II.

The various Figures, from No. 9 to No. 16 of this Plate, show the construction of an ash-pit door, with its register. (See page 31.)

Fig. 9 is a front view of the door with its register.—The whole is constructed of sheet-iron, except the four narrow pieces at the four corners, which hold down in its place the circular plate of the register; and the small circular plate (as large as an half-crown) in the center of the register, which are made of brass, on account of that metal not being so liable to rust as iron. (See page 31.)

Fig. 10 is a side view of the backside of the door, fixed in its frame, in which the manner of its being fast in its frame is seen, and the iron straps,

a, b, c, d, are seen, by which the frame is fastened in the brick-work.

Fig. 11 is an horizontal section through the middle of the door and its frame, and through the button which serves for shutting the door.

Fig. 12 is a section of this button, on an enlarged scale, showing the manner in which it is constructed.

Fig. 13 is the plate of sheet-iron which forms the front of the door, with the holes in it, by which the other parts of the machinery are fixed to it.

Fig. 14 is the circular plate which forms the register—to this plate is fixed a projecting knob, or button, (represented in the Figure,) by which it is turned about.

Fig. 15 and Fig. 16 show, on an enlarged scale, one of the four pieces of brass by which the circular plate of the register is kept down in its place.

In constructing these register doors, and in general all iron doors for fire-places, great and small, the door should never shut in a rabbet, or groove, in the frame, but should merely *shut down on the front edge of the frame*, which edge, by grinding it on the flat surface of a large flat stone, should be made quite level to receive it. If this be done, and if the plate of iron which constitutes the door be made quite flat, and if it be properly fixed on its hinges, the door will always shut with facility and close the opening with precision, notwithstanding
the

the effects of the expansion of the metal by heat ; but this cannot be the case when the doors of fire-places are fitted in grooves and rabbets.

Where the heat is very intense, the frame of the door should be made of fire-stone ; and that part of the door which is exposed naked to the fire should be covered either with a fit piece of fire-stone, fastened to it with clamps of iron, or a sufficient number of strong nails, with long necks and flat heads, or of staples, being driven into that side of the plate of iron which forms the door which is exposed, should be covered with a body about 2 inches thick of strong clay mixed with a due portion of coarse powder of broken crucibles, which mass will be held in its place by the heads of the nails and by the projecting staples. This mass being put on wet, and gently dried, the cracks being carefully filled up as they appear, and the whole well beaten together into a solid mass, will, when properly burned on by the heat of the fire, form a covering for the door which will effectually defend it from all injury from the fire ; and the door so defended will last ten times longer than it would last without this defence.

The inside doors of the two Brew-house Fire-places which I have fitted up at Munich are both defended from the heat in this manner ; and the contrivance, which has answered perfectly all that was expected from it, has not been found to be attended with any inconvenience whatever.

PLATE III.

Fig. 17 is a front view of the new boiler of the brew-house called Neuheufel, or rather of its fire-place and cover (the boiler being concealed in the brick-work). The inside door of the fire-place is here represented shut; and, in order that it might appear, the outside door is taken off its hinges, and is not shown. The two vaulted galleries, A, B, in the solid mass of the brick-work, on the right and left of the fire-place, (which were made to save bricks,) serve for holding fire-wood. The partition walls of the fire-place and the different flues,—as also a section of the boiler, are represented by dotted lines. The small circular hole on the left of the fire-place door is the window opening into the fire-place, by which the burning Fuel may be seen.

a, b, is the wooden curb of the boiler: *c, d*, a platform on which the men stand when they work in emptying the boiler, &c.: *e, f*, is a platform which serves as a passage from one side of the boiler to the other. This platform, which is about 18 inches wide, is 12 inches higher than the other platforms, in order that the openings *g* and *h*, into the flues, may remain free. These openings, which are opened only occasionally, that is to say, when the flues want cleaning, are kept closed by double brick-walls. These walls are expressed in the following Figure.

Fig. 18. This is a horizontal section of the fire-place at a level with the bottom of the boiler.

a, a,

a, a, a, a, are four openings by which the flues, which, in the first arrangement of this fire-place, went round the outside of the boiler, was occasionally cleaned : *b* is the canal by which the smoke went off into the chimney.

The entrance into the fire-place, and the conical perforation in the wall of the fire-place which serves as a window for observing the fire, are marked by dotted lines. The position of the outside door of the fire-place is marked by a dotted line, *c, d.* The circular dishing-grate is seen in its place ; and the walls of the flues under the boiler are all seen. The crooked arrows in the flues show the direction of the flame. (See page 100.)

PLATE IV.

Fig. 19 is a vertical section of the boiler represented in the foregoing Plate (Fig. 17.). This section is taken through the middle of the boiler, of the fire-place, and of the cover of the boiler. *A* is the ash-pit, with a section of its register door. *B* is the fire-place, and its circular dishing-grate. *C* is the entrance by which the Fuel is introduced, with sections of its two doors. *D* is a space left void to save bricks. *E* is the boiler, and *F* its wooden cover. *m* is the steam chimney, which is furnished with a damper. *R, R,* is the vertical wall of the house against which the brick-work in which the boiler is fixed is placed.

a, b, is the curb of timber in which the boiler is set.

The

The manner in which the cover of the boiler is constructed, as well as its form, and the door and windows which belong to it, are all seen distinctly in this Figure.

Fig. 20 is an horizontal section of this fire-place taken on a level with the bottom of the flue which goes round the outside of the boiler, in which flue, before the fire-place was altered, the flame circulated. The flues under the boiler are, in this Figure, indicated by dotted lines.

PLATE V.

Fig. 21 is a horizontal section of the fire-place of the brewhouse boiler, at a level with the top of the flues under the boiler, *after the flue round the outside of the boiler had been stopped up*, or rather the flame prevented from circulating in it. This Figure shows the actual state of the fire-place at the present time. (See page 126.)

The crooked arrows show the direction of the flame in the flues. — *a, b*, are the two canals (each of which is furnished with a damper) by which the smoke goes off into the chimney; — and *c, c, c, c, c, c*, are six small openings communicating with the flues, by which the flame and hot vapour can pass up into the cavity on the outside of the boiler which formerly served as a flue.

Fig. 22 is a front view of the ash-pit door of this brewhouse fire-place, with its register. This door is closed by means of a latch of a particular construction, which is shown in the Figure.

Fig. 23 is the door without its register;—and

Fig. 24 the circular plate of the register represented alone.

This ash-pit door shuts against the front edge of its frame, and not into it; the reasons for preferring this method of fitting the door to its frame have already been explained. (See descriptions of the Plate II.)

PLATE VI.

Fig. 25 is a section of a small lime-kiln, built, or rather now building, at Munich, for the purpose of making experiments. The height of the kiln is 15 feet;—its internal diameter below, 2 feet,—and above, 9 inches. In order more effectually to confine the heat, its walls, which are of bricks and very thin, are double, and the cavity between them is filled with dry wood ashes. To give greater strength to the fabric, these two walls are connected in different places by horizontal layers of bricks which unite them firmly.

a, is the opening by which the fuel is put into the fire-place. Through this opening the air *descends* which feeds the fire. The fire-place is represented nearly full of coals, and the flame passing off laterally into the cavity of the kiln, by an opening made for that purpose at the bottom of the fire-place.

The opening above, by which the fuel is introduced into the fire-place, is covered by a plate of iron,

iron, moveable on hinges; which plate, by being lifted up more or less by means of a chain, serves as a register for regulating the fire.

A section of this plate, and of the chain by which it is supported, are shown in the Figure.

b is an opening in the front wall of the fire-place, which serves occasionally for cleaning out the fire-place, and the opening by which the flame passes from the fire-place into the kiln. This opening, which must never be quite closed, serves likewise for admitting a small quantity of air to pass horizontally into the fire-place. A small proportion of air admitted in this manner has been found to be useful and even necessary in fire-places in which, in order to consume the smoke, the flame is made to descend. Several small holes for this purpose, fitted with conical stoppers, may be made in different parts of the front wall of the fire-place.

The bottom of the fire-place is a grate constructed of bricks placed edgewise, and under this grate there is an ash-pit; but as no air must be permitted to pass up through this grate into the fire-place, the ash-pit door, *c*, is kept constantly closed, being only opened occasionally to remove the ashes.

d is the opening by which the lime is taken out of the kiln; which opening must be kept well closed, in order to prevent a draught of cold air through the kiln.

As only as much lime must be removed at once as is contained in that part of the kiln which lies below the level of the bottom of the fire-place, to be able to ascertain when the proper quantity is taken

taken away, the lime, as it comes out of the kiln, may be directed into a pit sunk in the ground in front of the opening by which the lime is removed, this pit being made of proper size to serve as a measure.

While the lime is removing from the bottom of the kiln, fresh lime-stone should be put into it above; and, during this operation, the fire may be damped by closing the top of the fire-place with its iron plate.

Should it be found necessary, the fire, and the distribution of the heat, may, in burning the lime, be farther regulated by closing more or less the opening at the top of the lime-kiln with a flat piece of fire-stone, or a plate of cast iron.

The double walls of the kiln, and the void space between them, as also the horizontal layers of bricks by which they are united, are clearly and distinctly expressed in the Figure. The kiln is represented as being nearly filled with small round stones, such as are used at Munich in burning lime. These stones are brought down from the calcareous mountains on our frontiers, by the river (the Isar), and are rounded by rubbing against each other as they are rolled along by the impetuosity of the torrent.

END OF THE SIXTH ESSAY.

Fig. 1.



Fig. 2.



Fig. 3.



Fig. 4.



Fig. 5.



Fig. 6.



Fig. 7.



Fig. 8.



Took on Strand.

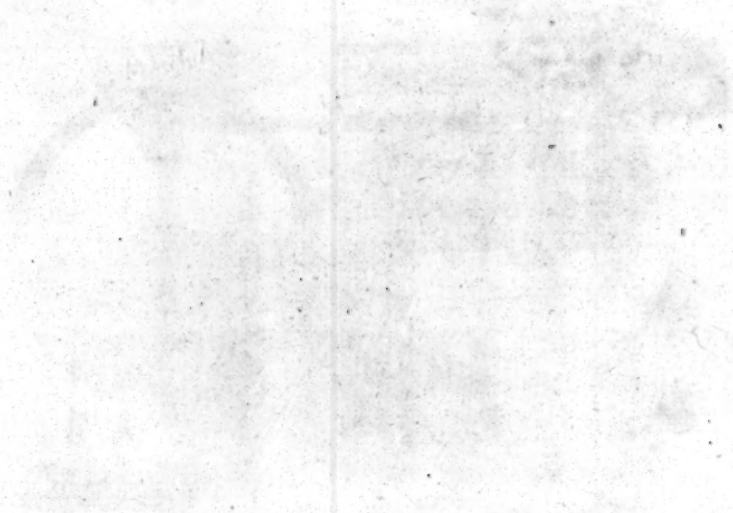
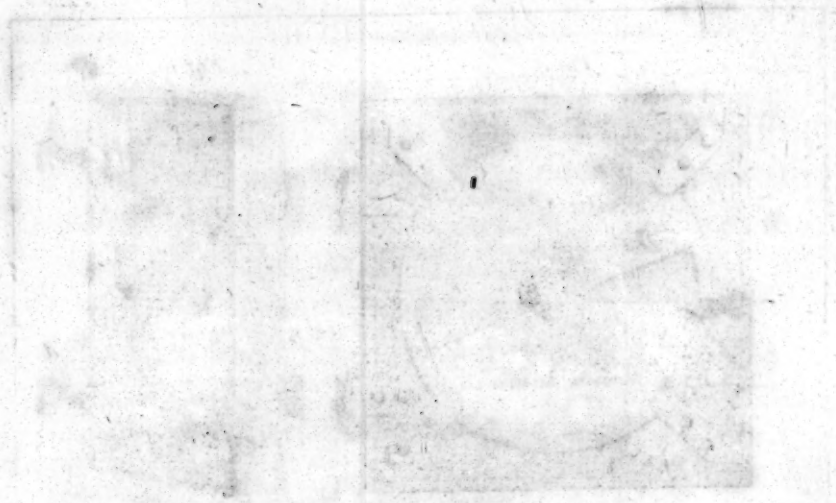


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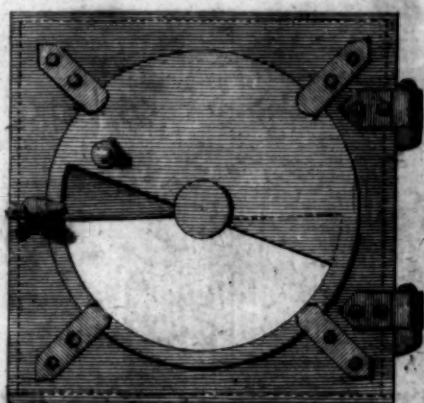


Fig. 10.

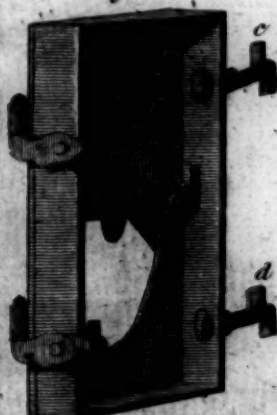


Fig. 11.



Fig. 12.



Fig. 13.

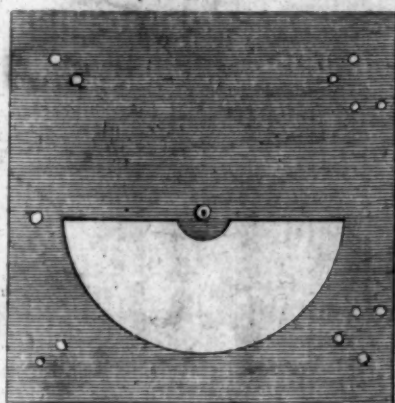


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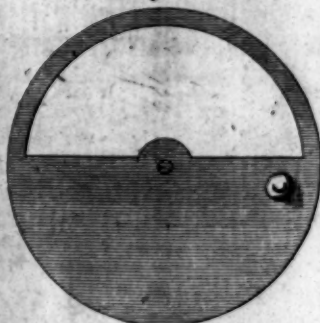


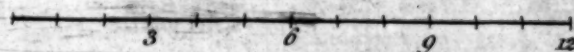
Fig. 15.



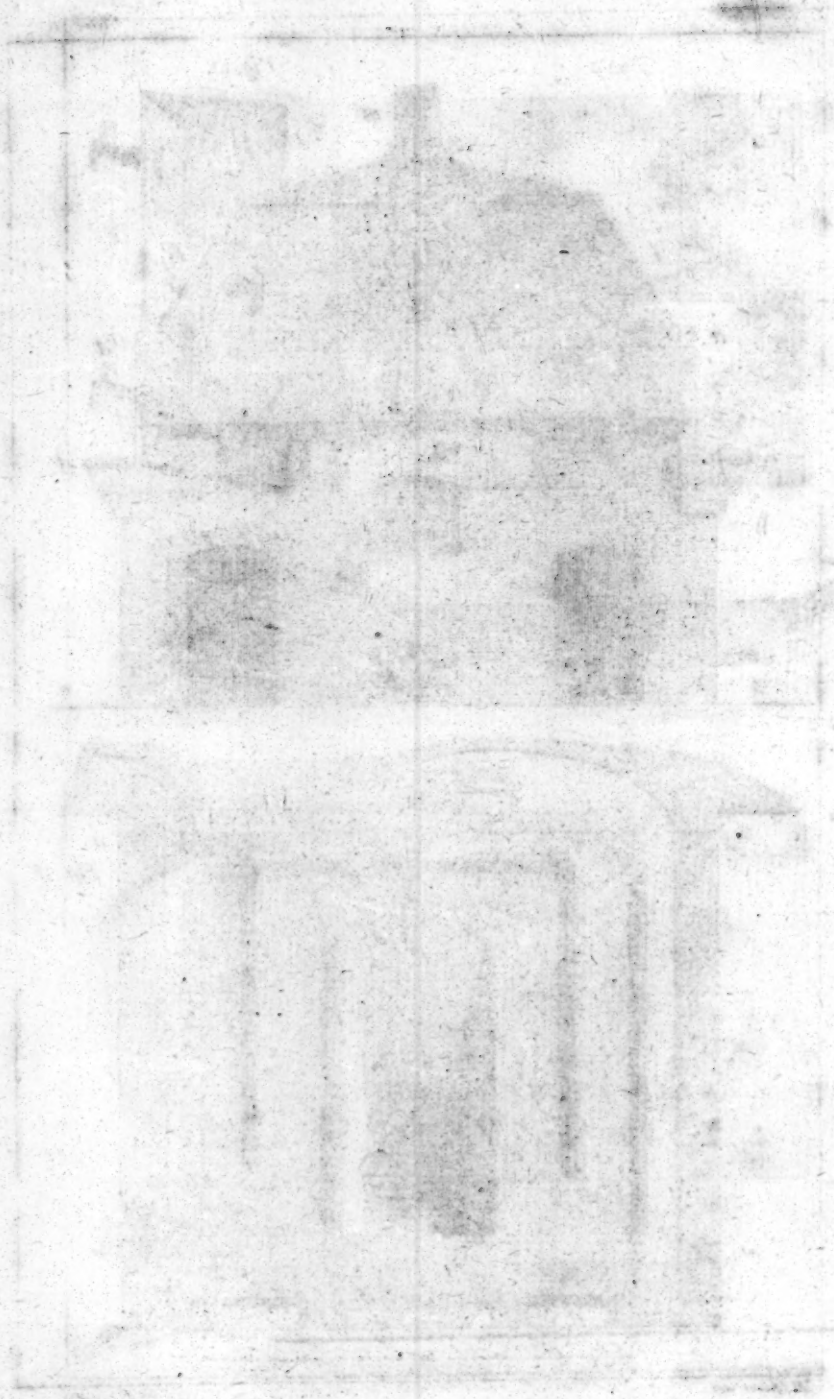
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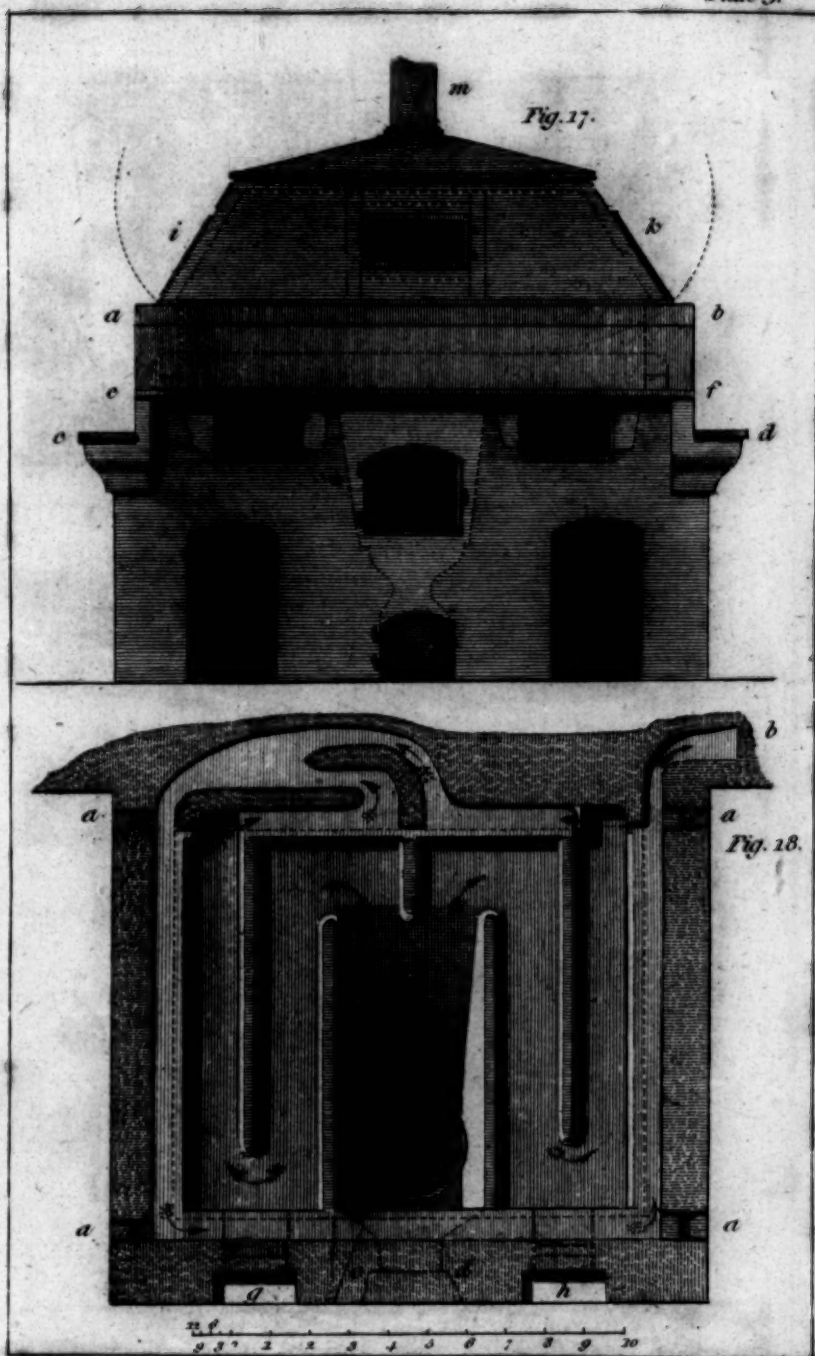


Scale of Inches



Scale as shown.





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Jacob de Stroud.

London Pub.^d. April 15.th 1797 by Cuttell & Davis Seniors.

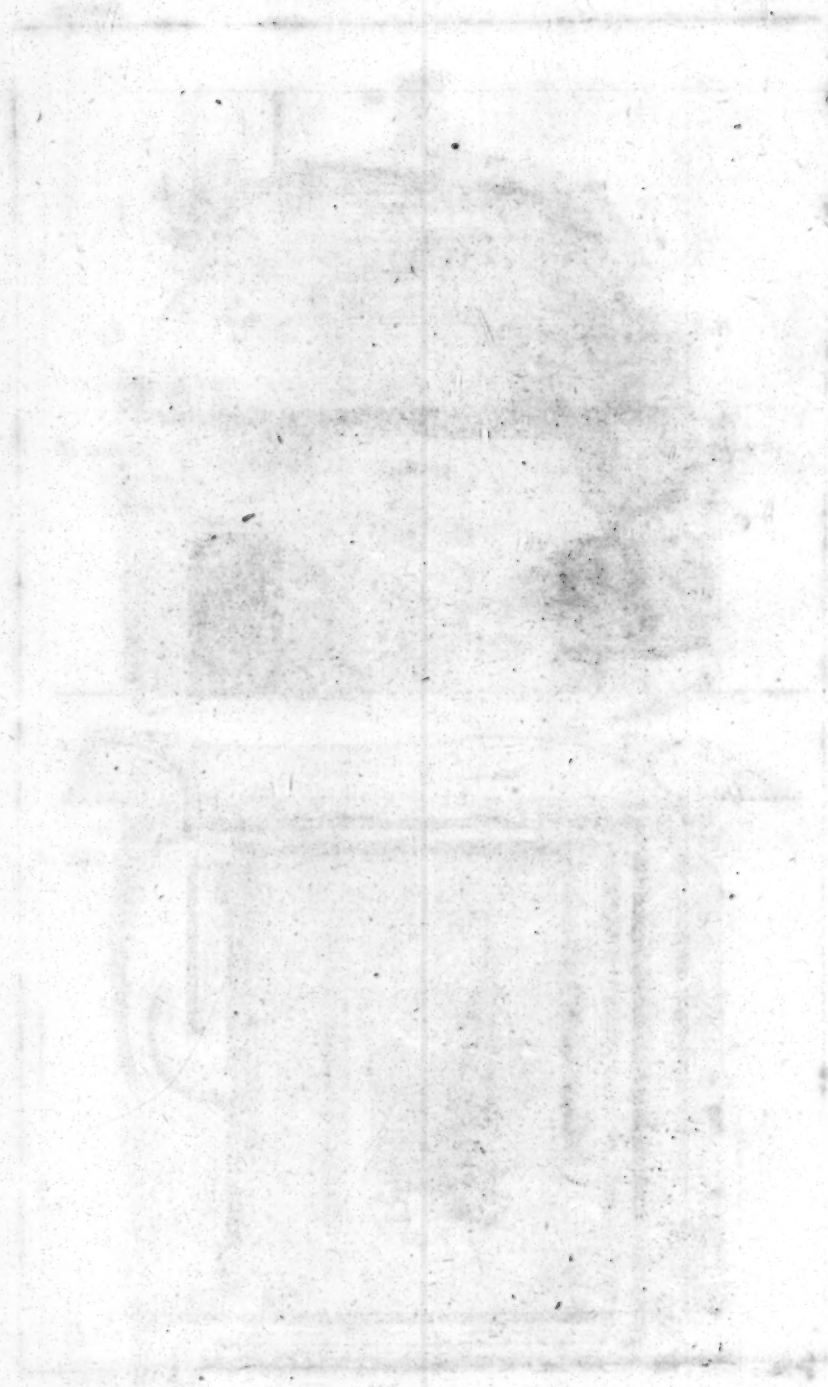


Fig. 19.

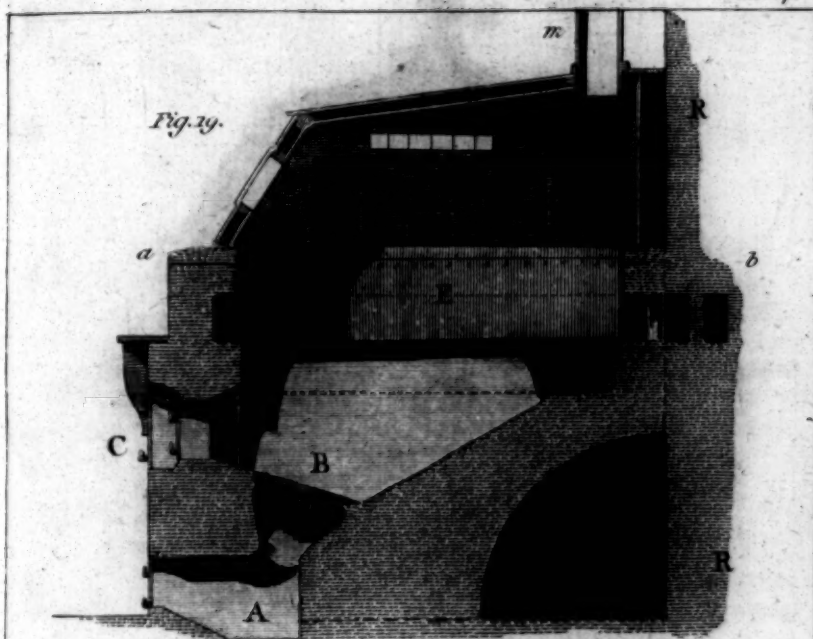


Fig. 20.

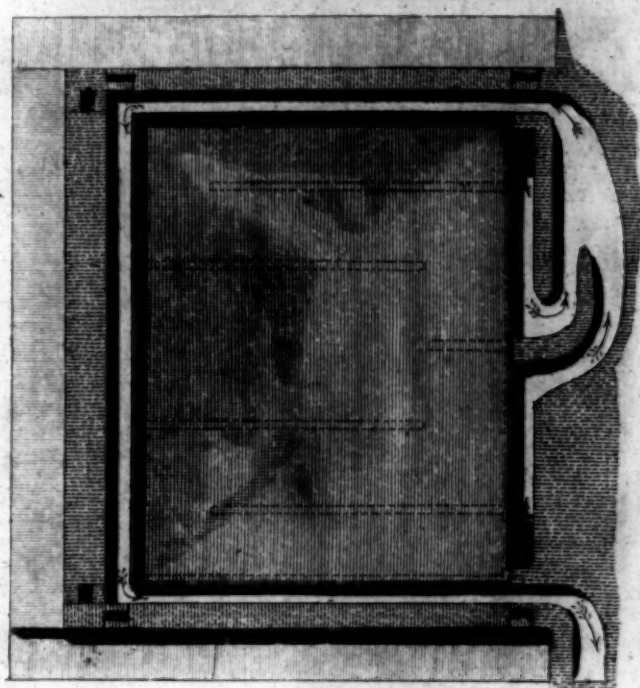


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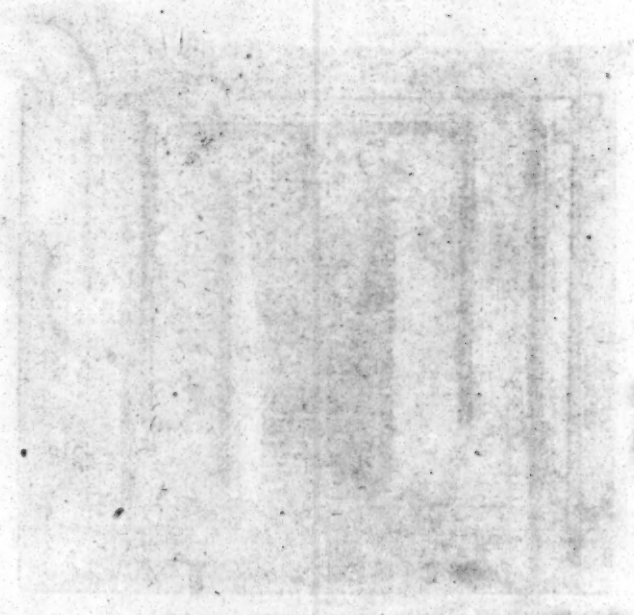
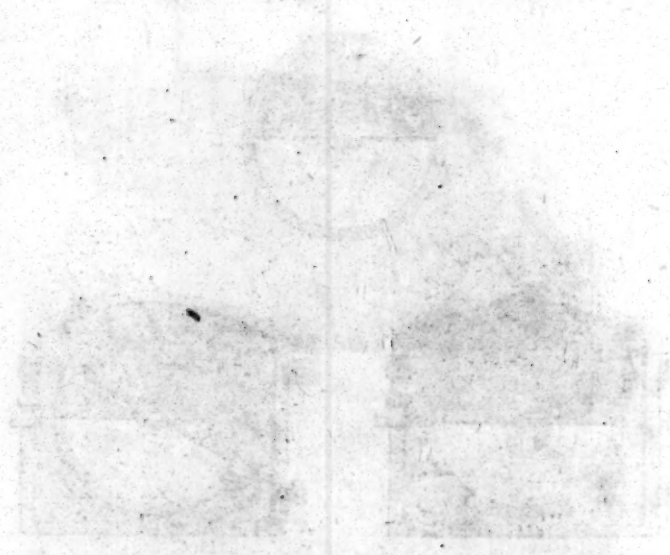


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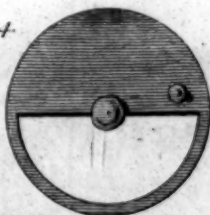


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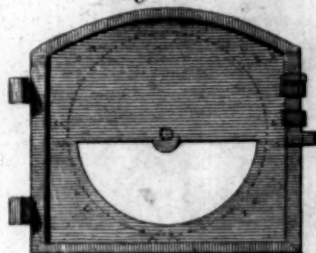
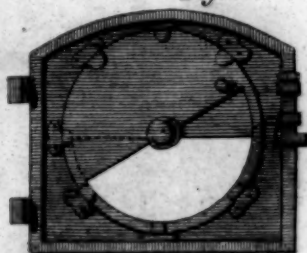
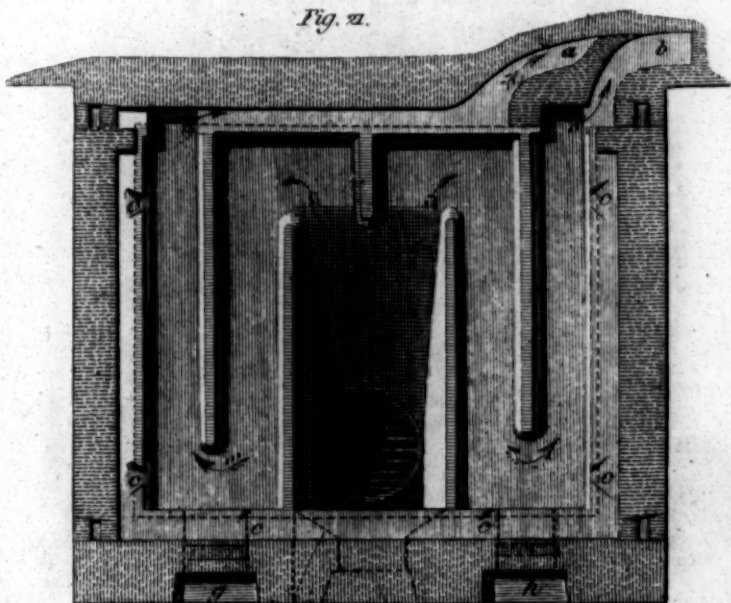


Fig. 22.



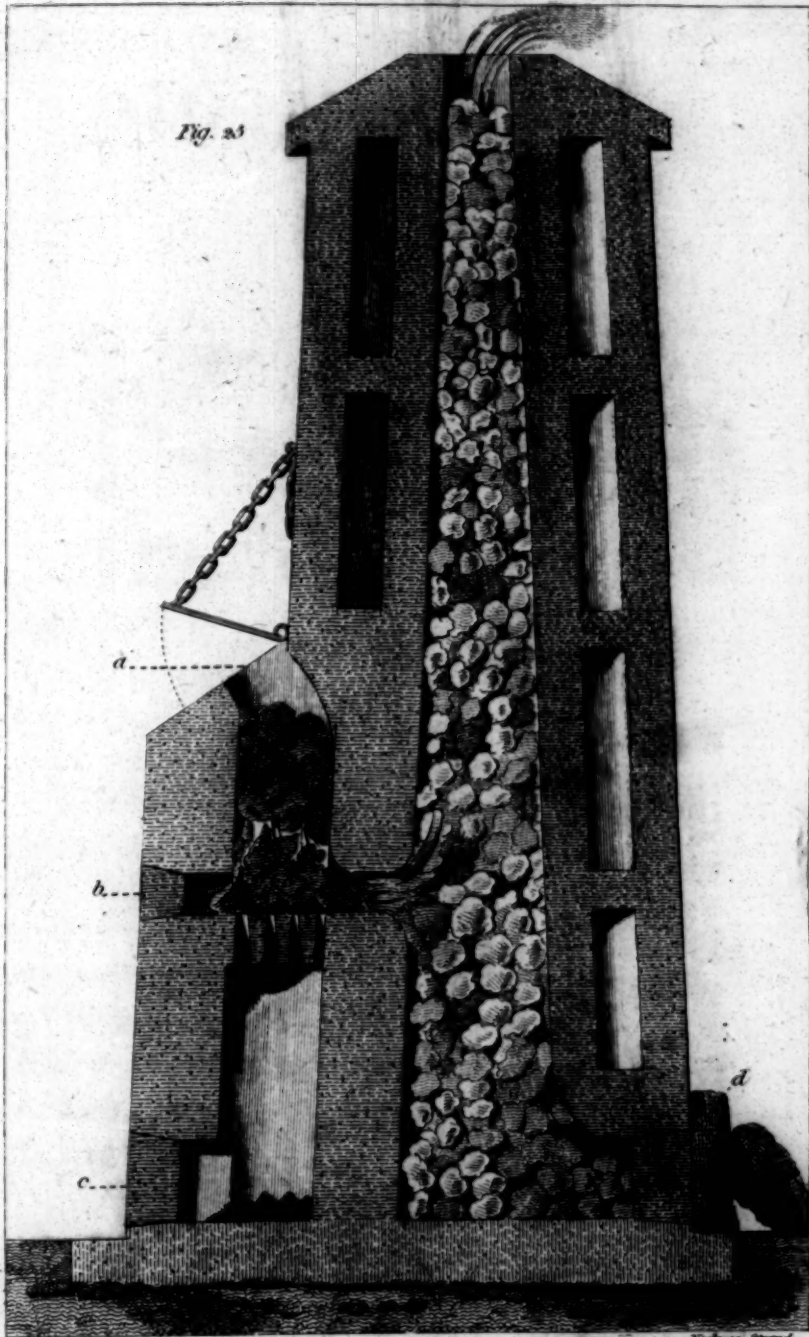
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Fig. 21.



Scale of Strands.

Fig. 23



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TO THE

SEVENTH ESSAY.

THE Author finds himself once more in a situation, in which he feels it to be his duty to apologize for having deviated from the plan announced to the Public in respect to the order of his Publications.—The Treatise on the Construction of Kitchen Fire-places, and Kitchen Utensils, which was promised some time ago, and which, according to the original plan of the work, ought to have constituted the *Seventh* Essay, is not yet ready for the press;—a desire to render it as perfect as possible having induced the Author to delay its publication, in order to wait the results of some new Experiments which he is now engaged in making, and which, he has reason to hope, will lead to farther improvements in the Management of Heat in culinary processes.

The object principally had in view in these Experiments, was to contrive portable closed fire-places to serve instead of fixed fire-places for cooking on a small scale; or which might be used

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occasionally in the kitchens of private families when food is provided for a much larger number of persons than usual.

Such portable fire-places could not fail to be extremely useful to poor families, who cook their victuals in the rooms in which they live; and they would certainly be very convenient in the kitchens of the opulent, which, in order that dinners may occasionally be prepared for large companies, are commonly made much more spacious and expensive than otherwise would be necessary.

Another object had in view, and which is certainly of the most serious importance, was to contrive matters so that sauce-pans, and other kitchen-utensils constructed of porcelain and of earthenware, might, without any inconvenience, be used in preparing food, instead of those now in common use, which are mostly of copper, by which the deleterious effects of that poisonous metal may be avoided.

The Author is now fitting up a large kitchen, (in the new Veterinary College in the English garden at Munich,) in the construction of which there is not a particle of any kind of metal employed; and all the sauce-pans and smaller boilers belonging to it are of earthenware. He is fitting up another, (in his own house,) in which the sauce-pans and cassaroles are all of white porcelain, made very thin,—free from all sharp edges,—and covered on the outside with thin sheet-iron, to prevent the effects of a too sudden application of heat.

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heat. How far these various contrivances may deserve to be recommended as models for imitation, will soon be determined by actual Experiments; and a faithful account of the results of these Experiments shall, as soon as possible, be laid before the Public.

There is another Experiment, of a different kind, in which the Author is engaged, (the result of which he means in due time to publish,) which cannot fail to interest very deeply many of his benevolent readers. He is forming a public establishment, on a plan which is, in many respects, entirely new, for the education of 100 poor children of both sexes, from five to six years of age; and he hopes to be able to *prove*, by this Experiment, that children of that age may be maintained, well educated, and rendered useful members of society, without any expence whatever, either to their parents or to the Public. To render them strong, healthy, and cheerful, they will be made to take a great deal of exercise in the open air; much of their time will be employed in cultivating a farm, and a large kitchen-garden; and they will be allowed a reasonable time for their recreations.—They will be instructed in agriculture and in gardening, and in all the various occupations of rural life; and in bad weather, and particularly in winter, they will be employed in various manufactures; such as can be carried on within doors, and are not detrimental to health. To encourage in the most effectual manner a spirit
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of industry and a habit of œconomy among the children belonging to this establishment, the whole of their earnings, over and above the necessary expences of their maintenance, will belong to themselves; and a separate and exact account will be kept with each of them, from the time they enter the establishment, till they quit it; and when they go away, the balance due to them will be faithfully paid into their hands.

From the accounts which have been collected of the earnings of the children who frequent the House of Industry at Munich, and the estimates which have been made of the expence of their maintenance, there appears to be the strongest reasons to think that this scheme can hardly fail to be attended with success.

ESSAY VII.

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FLUIDS.

OF A

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with COLD, when it is near the Tempe-
rature at which it freezes; and of the
wonderful Effects which are produced by
the Operation of that LAW, in the
Economy of Nature.

TOGETHER WITH

Conjectures respecting the FINAL CAUSE of
the SALTNESS OF THE SEA.

ESSAY

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E S S A Y VII.

C H A P. I

Danger of admitting received Opinions in Philosophical Investigations, without Examination.—The free Passage of HEAT, in all Bodies, in all Directions, never yet called in question.—Heat does not, however, pass in this Manner, in all Bodies without Exception.—AIR and WATER, and probably all other FLUIDS, are, in fact, NON-CONDUCTORS OF HEAT.—Accidental Discoveries, which led to an experimental Investigation of this curious Subject.—The internal Motions among the Particles of Fluids rendered visible.—The Propagation of Heat in Fluids obstructed and retarded, by every thing which obstructs the internal Motions of their Particles ;—hence there is Reason to conclude, that Heat is propagated in them, only in consequence of those Motions ;—that it is transported by them,—not suffered to pass through them.—FURS and FEATHERS, and all other like Substances, which, in Air, form warm Covering for confining Heat, found, by Experiment, to produce the same Effects in Water.—These Effects are probably produced in both Fluids in the same Manner, namely, by obstructing the

Motions of their Particles, in the Operation of transporting the Heat.—The conducting Power of Water, remarkably impaired by mixing with it such Substances as render it viscous, and diminish its Fluidity.—These Discoveries respecting the Manner in which Heat is propagated in Water, throw much Light on several of the most interesting Operations in the Economy of Nature.—They enable us to account, in a satisfactory Manner, for the Preservation of Trees and other Vegetables, and of Fruits, during the Winter, in cold Climates.

IT is certain, that there is nothing more dangerous, in philosophical investigations, than to take any thing for granted, however unquestionable it may appear, till it has been proved by direct and decisive experiment.

I have very often, in the course of my philosophical researches, had occasion to lament the consequences of my inattention to this most necessary precaution.

There is not, perhaps, any phænomenon that more frequently falls under our observation, than the Propagation of Heat. The changes of the temperature of sensible bodies—of solids—liquids—and elastic fluids, are going on perpetually under our eyes; and there is no fact which one would not as soon think of calling in question, as to doubt of the free passage of Heat, in all directions, through all kinds of bodies. But, however obviously this conclusion appears to flow, from all that we observe and experience in the common course
of

of life, yet it is certainly not true;—and to the erroneous opinion respecting this matter, which has been universally entertained—by the *learned*, and by the *unlearned*—and which has, I believe, never even been called in question, may be attributed the little progress that has been made in the investigation of the science of Heat:—a science, assuredly, of the utmost importance to mankind!

Under the influence of this opinion, I, many years ago, began my experiments on Heat; and had not an accidental discovery drawn my attention with irresistible force, and fixed it on the subject, I probably never should have entertained a doubt of the free passage of Heat *through air*; and even after I had found reason to conclude, from the results of experiments which to me appeared to be perfectly decisive, that air is a *non-conductor* of Heat; or that Heat cannot pass through it, without being transported by its particles; which, in this process, act individually, or independently of each other; yet, so far from pursuing the subject, and contriving experiments to ascertain the manner in which Heat is communicated in other bodies, I was not sufficiently awakened to suspect it to be even possible, that this quality could extend farther than to elastic Fluids.

With regard to liquids, so entirely persuaded was I, that Heat could pass freely, *in them*, in all directions, that I was perfectly blinded by this prepossession, and rendered incapable of seeing the most striking and most evident proofs of the fallacy of this opinion.

I have already given an account, in one of my late publications—(Essay VI. on the Management of Fire, and the Economy of Fuel)—of the manner in which I was led to discover, that *steam* and *flame* are *non-conductors* of Heat:—I shall now lay before the Public an account of a number of experiments I have lately made, which seem to shew that *water*,—and probably all other liquids,—and Fluids of every kind, possess the same property. That is to say, that although the particles of any Fluid, *individually*, can receive Heat from other bodies, or communicate it to them; yet, among these particles themselves, all *interchange* and *communication* of Heat is absolutely impossible.

It may, perhaps, be thought not altogether uninteresting, to be acquainted with the various steps by which I was led to an experimental investigation of this curious subject of enquiry.

When dining, I had often observed that some particular dishes retained their Heat much longer than others; and that apple-pies, and apples and almonds mixed,—(a dish in great repute in England,)—remained hot a surprising length of time.

Much struck with this extraordinary quality of retaining Heat, which apples appeared to possess, it frequently occurred to my recollection; and I never burnt my mouth with them, or saw others meet with the same misfortune, without endeavouring, but in vain, to find out some way of accounting, in a satisfactory manner, for this surprising phenomenon.

About four years ago, a similar accident awakened my attention, and excited my curiosity
still

still more : being engaged in an experiment which I could not leave, in a room heated by an iron stove, my dinner, which consisted of a bowl of thick rice-soup, was brought into the room ; and as I happened to be too much engaged at the time to eat it, in order that it might not grow cold, I ordered it to be set down on the top of the stove : about an hour afterwards, as near as I can remember, beginning to grow hungry, and seeing my dinner standing on the stove, I went up to it, and took a spoon-full of the soup, which I found almost cold, and quite thick. Going, by accident, deeper with the spoon the second time, this second spoon-full burnt my mouth *. This accident recalled very forcibly to my mind the recollection of the hot apples and almonds, with which I had so often burned my mouth, a dozen years before, in England ; but even this, though it surprised me very much, was not sufficient to open my eyes, and to remove my prejudices respecting the conducting power of water.

Being at NAPLES, in the beginning of the year 1794, among the many natural curiosities which attracted my attention, I was much struck with several very interesting phænomena which the hot baths of BAIA presented to my observation ; and among them there was one, which quite astonished

* It is probable that the stove happened to be nearly cold when the bowl was set down upon it, and that the soup had grown almost cold ; when, a fresh quantity of fuel being put into the stove, the Heat had been suddenly increased.

me: standing on the sea-shore, near the baths, where the hot steam was issuing out of every crevice of the rocks, and even rising up out of the ground, I had curiosity to put my hand into the water. As the waves which came in from the sea followed each other without intermission, and broke over the even surface of the beach, I was not surprised to find the water cold;—but I was more than surprised, when, on running the ends of my fingers through the cold water into the sand, I found the heat so intolerable, that I was obliged instantly to remove my hand. The sand was perfectly wet; and yet, the temperature was so very different at the small distance of two or three inches! I could not reconcile this with the supposed great conducting power of water.—I even found that the top of the sand was, to all appearance, quite as cold as the water which flowed over it; and this increased my astonishment still more. I then, for the first time, began to doubt of the conducting power of water, and resolved to set about making experiments to ascertain the fact; I did not however put this resolution into execution till about a month ago; and should perhaps never have done it, had not another unexpected appearance again called my attention to it, and excited afresh all my curiosity.

In the course of a set of experiments on the communication of Heat, in which I had occasion to use thermometers of an uncommon size,—(their globular bulbs being above four inches in diameter,)

ter,)—filled with various kinds of liquids, having exposed one of them, which was filled with spirits of wine, in as great a heat as it was capable of supporting, I placed it in a window, where the sun happened to be shining, to cool; when, casting my eye on its tube, which was quite naked,—(the divisions of its scale being marked in the glass with a diamond,)—I observed an appearance which surprised me, and at the same time interested me very much indeed. I saw the whole mass of the liquid in the tube in a most rapid motion, running swiftly in two opposite directions, *up*, and *down*, at the same time. The bulb of the thermometer, which is of copper, had been made two years before I found leisure to begin my experiments; and having been left unfilled, without being closed with a stopple, some fine particles of dust had found their way into it, and these particles, which were intimately mixed with the spirits of wine, on their being illuminated by the sun's beams, became perfectly visible,—(as the dust in the air of a darkened room is illuminated and rendered visible by the sun-beams which come in through a hole,)—and by their motion discovered the violent motions by which the spirits of wine in the tube of the thermometer was agitated.

This tube, which is $\frac{4}{100}$ of an inch in diameter internally, and very thin, is composed of very transparent, colourless glass, which rendered the appearance clear and distinct, and exceedingly beautiful. On examining the motion of the spirits of wine with a lens, I found that the ascending
current

current occupied the *axis of the tube*, and that it descended by the *sides of the tube*.

On inclining the tube a little, the *rising* current moved out of the axis, and occupied that side of the tube which was uppermost, while the *descending* current occupied the whole of the lower side of it.

When the cooling of the spirits of wine in the tube was hastened by wetting the tube with ice-cold water, the velocities of both the ascending and the descending currents were sensibly accelerated.

The velocity of these currents was gradually lessened, as the thermometer was cooled; and when it had acquired nearly the temperature of the air of the room, the motion ceased entirely.

By wrapping up the bulb of the thermometer in furs, or any other warm covering, the motion might be greatly prolonged.

I repeated the experiment with a similar thermometer of equal dimensions, filled with linseed-oil, and the appearances, on setting it in the window to cool, were just the same. The directions of the currents, and the parts they occupied in the tube, were the same; and their motions were to all appearance quite as rapid as those in the thermometer which was filled with spirits of wine.

Having now no longer any doubt with respect to the cause of these appearances, being persuaded that the motion in these liquids was occasioned by their particles *going individually*, and *in succession*, to give off their Heat to the cold side of the tube, in the same manner as I have shown in another

place, that the particles of air give off *their* Heat to other bodies, I was led to conclude that these, and probably all other liquids, are in fact *non-conductors* of Heat; and I went to work immediately to contrive experiments to put the matter out of all doubt.

On considering the subject attentively, it appeared to me, that if liquids were in fact *non-conductors* of Heat, or if it be propagated in them *only* in consequence of the internal motions of their particles; in that case, every thing which tends to obstruct those motions, ought certainly to retard the operation, and render the propagation of the Heat slower, and more difficult. I had found that this is actually the case in respect to air; and though (under the influence of a strong and deep-rooted prejudice) I had, from the result of one imperfect experiment, too hastily concluded, that it did not take place in regard to water; yet I now found strong reasons to call in question the result of that experiment, and to give the subject a careful and thorough investigation.

Thinking that the best mode of proceeding, in this enquiry, would be to adopt a method similar to that I had pursued in my experiments on the conducting power of Air, I prepared an apparatus suitable to that purpose. The first object I had in view being to discover whether the propagation of Heat through water was obstructed, or not, by rendering the internal motion among the particles of the water, occasioned by their change of temperature, embarrassed and difficult, I contrived to
make

make a certain quantity of Heat pass through a certain quantity of pure water, confined in a certain space; and noting the time employed in this operation, I repeated the experiment again, with the same apparatus, with this difference only, that in this second trial, the water through which the Heat was made to pass, instead of being pure, was mixed with a small quantity of some fine substance, (such as *ider-down*, for instance)—which, without altering any of its chemical properties, or impairing its fluidity, served merely to obstruct and embarrass the motions of the particles of the water in transporting the Heat, in case Heat should be actually *transported* or *carried* in this manner, and not suffered to pass freely through liquids.

The body which received the Heat, and which served, at the same time, to measure the quantity of it communicated, was a very large cylindrical thermometer. (See Plate I.) The bulb of this thermometer, which is constructed of thin sheet-copper, is cylindrical, its two ends being hemispheres.

Its dimensions are as follows:

Dimensions of the bulb of the thermometer.	{	Diameter	-	1.84 inches.
		Length	-	4.99 —
		Capacity or contents	-	13.2099 cubic inches.
		External superficies	-	28.834 superficial inches.

The thickness of the sheet-copper of which it is constructed, is 0.03 of an inch. It weighs, empty, 1846 grains;—and is capable of containing 3344 grains of water, at the temperature of 55°. This copper-bulb has a glass tube, 24 inches long, and

$\frac{1}{8}$ of

$\frac{1}{16}$ of an inch in diameter; which is fitted by means of a good cork, into a cylindrical tube or neck of copper, one inch long, and $\frac{63}{100}$ of an inch in diameter, belonging to the metallic bulb.

This thermometer, being filled with linseed-oil, and its scale graduated, was fixed in the axis of a hollow cylinder, constructed of thin sheet-copper, $11\frac{1}{2}$ inches long; and 2.3435 inches in diameter internally. This cylinder, which is open at one end, is closed at the other with a hemispherical bottom, with its convex surface outwards. The cylinder weighs 2261 grains, and the sheet-brass of which it is constructed is, 0.0128 of an inch in thickness.

The bulb of the thermometer was placed in the lower part of this brass cylindrical tube, and was confined in the middle, or axis of it, by means of three pins of wood, about $\frac{1}{8}$ of an inch in diameter, and $\frac{1}{4}$ of an inch long, which pins are fixed in tubes of thin sheet brass, $\frac{1}{8}$ of an inch in diameter, and $\frac{3}{16}$ of an inch in length. These short tubes, which are placed at proper distances on the inside of the large brass tube, at its lower end, and firmly attached to it by solder, serve as sockets into which the ends of the wooden pins, which, pointing inwards, or towards the axis of the large cylindrical tube, serve to confine the lower end of the bulb of the thermometer in its proper place. Its upper end is kept in its place; or the axis of the thermometer is made to coincide with the axis of the brass cylinder, by causing the tube of the thermometer to pass through a hole in the

the

the middle of a cork stopper which closes the end of the cylinder.

The bottom of the bulb of the thermometer does not repose on the hemispherical bottom of the brass cylinder, but is supported at the distance of $\frac{1}{4}$ of an inch above it, on the end of a wooden pin, like those just described; which pin is fixed in a socket in the middle of the bottom of the cylindrical tube, and projects upwards. The ends of all these wooden pins which project beyond the sockets in which they are fixed, are reduced to a blunt point: This was done to reduce as much as possible the points of contact between the ends of these pins and the bulb of the thermometer.

The thermometer being in its place, there is on every side a void space left between the bulb of the thermometer and the internal surface of the brass cylinder in which it is confined; the distance between the external surface of the bulb of the thermometer and the internal surface of the containing cylinder being 0.25175 of an inch. This space is designed to contain the water and other substance through which the Heat is made to pass *into*, or *out of*, the bulb of the thermometer; and the quantity of Heat which has passed is shewn by the height of the fluid in the tube of the thermometer. The quantity of water required to fill this space and to cover the upper end of the bulb of the thermometer to the height of about $\frac{1}{4}$ of an inch was found to weigh 2468 grains. As the thermometer was plunged into this water it was of course in contact with it by its whole surface, which, as we have seen, is equal to 28.834 square inches.

The

The bulb of the thermometer being surrounded by water, or by any other liquid, or mixture, the conducting power of which was to be ascertained, a cylinder of cork something less in diameter than the brass cylinder,—about half an inch long, with a hole in its centre, in which the tube of the thermometer passed freely, was thrust down into the brass cylinder, but not quite so low as to touch the surface of the water, or other substance it contained. This cylinder, or disk, was supported in its proper place by three projecting brass points or pins which were fixed with solder to the outside of the metallic neck of the bulb of the thermometer.

As soon as this disk of cork is put into its place, the upper part of the hollow brass cylindrical tube is filled with eider-down, and it is closed above with its cork stopper, the tube of the thermometer, which passes through a fit hole in the middle of this stopper, projecting upwards. As the whole scale of the thermometer, from the point of freezing to that of boiling water, is above the upper surface of this stopper, all the changes of heat to which the instrument is exposed, can be observed at all times without deranging any part of the apparatus.

The thermometer is divided according to the scale of Fahrenheit, and its divisions are made to correspond with a very accurate mercurial thermometer made by TROUGHTON.

The experiments with this instrument, which, for the sake of distinction, I shall call my *cylindrical passage*

passage thermometer, were made in the following manner: The thermometer being fixed in its cylindrical brass tube, in the manner above described, and surrounded by the substance the conducting power of which was to be ascertained, the instrument was placed in thawing ice, where it was suffered to remain till the thermometer fell to 32° . It was then taken out of the melting ice, and immediately plunged into a large vessel of boiling water, and the conducting power of the substance which was the subject of the experiment was estimated by the time employed by the Heat in passing through it into the thermometer; the time being carefully noted when the liquid in the thermometer arrived at the 40th degree of its scale; and also when it came to every 20th degree above it.

As the slower Heat moves, or is transported, in any medium, the longer must of course be the time required for any given quantity of it to pass through it; and as the thermometer shows the changes which take place in the temperature of the body which is heated or cooled,—(namely, the liquid with which the thermometer is filled,)—in consequence of the passage of the Heat through the medium by which the thermometer is surrounded, the conducting power of that medium is shown by the quickness of the ascent or descent of the thermometer, when, having been previously brought to a certain temperature, the instrument is suddenly removed and plunged into another medium at any other constant given temperature.

Having still fresh in my memory the accidents I had so often met with in eating hot Apple-pies, I was very impatient, when I had completed this instrument, to see if Apples, which, as I well knew, are composed almost entirely of water, really possess a greater power of retaining Heat than that liquid when it is pure, or unmixed with other bodies. But before I made the Experiment, in order that its result might be the more satisfactory, I determined, in the following manner, how much water there really is in Apples, and what proportion their fibrous parts bear to their whole volume.

960 Grains of stewed Apples (the Apples having been carefully pared and freed from their stems and seeds before they were stewed) were well washed in a large quantity of cold spring water, and the fibrous parts of the Apples being suffered to subside to the bottom of the vessel, the clear part of the liquor was poured off, and the fibrous remainder being thoroughly dried was carefully weighed, and was found to weigh just 25 grains.

This fibrous remainder of the 960 grains of stewed Apples being again washed in a fresh quantity of cold spring water, and afterwards very thoroughly dried, by being exposed several days on a china plate placed on the top of a German stove, which was kept constantly hot, was again weighed, and was found to weigh no more than $18\frac{2}{3}$ grains.

From this Experiment it appears that the fibrous parts of stewed Apples amount to less than $\frac{1}{16}$ part

of the whole mass; and there is abundant reason to conclude that the remainder, amounting to $\frac{4}{5}$ of the whole, is little else than pure water.

Having surrounded the bulb of my cylindrical passage thermometer with a quantity of these stewed Apples, (the consistence of the mass being such that it shewed no signs of fluidity,) the instrument was placed in pounded ice, which was melting, and when the thermometer indicated that the whole was cooled down to the temperature of 32° , the instrument was taken out of the melting ice, and plunged into a large vessel of boiling water, and the water being kept boiling with the utmost violence during the whole time the Experiment lasted, the times taken up in heating the thermometer from 20 to 20 degrees were observed and noted down in a table, which had been previously prepared for that purpose.

This Experiment having been repeated twice, and varied as often by first heating the instrument to the temperature of boiling water, and then plunging it into melting ice, and observing the time taken up in the passage of the heat *out* of the thermometer; I removed the stewed Apples which surrounded the bulb of the thermometer, and filling the space they had occupied with *pure water*, I now repeated the Experiments again with that liquid. The following Tables show the results of these Experiments.

Time the Heat was passing INTO the Thermometer,				
Through stewed Apples.		Through Water.		
Exp. No. 1.	Exp. No. 3.	Exp. No. 5.	Exp. No. 7.	
Seconds.	Seconds.	Seconds.	Seconds.	
In heating the Thermometer from the temperature of 32° to that of 40° } from 40° to 60° 60° to 80° 100° 120° 140° 160° 180° 200°	95	89	45	45
	75	67	36	35
	61	56	34	31
	65	60	30	30
	73	66	37	36
	90	82	44	44
	121	113	63	60
	188	170	93	90
	360	364	226	215
Total times in heating from 32° to 200° }	1128	1057	608	586
Times employed in heating the instrument 80 degrees, viz. from 80° to 160° }	349"	321"	174"	170"
Mean times in heating it from 80° to 160° }	In stewed Apples 335"		In Water 172"	

The results of these Experiments show that Heat passes with much greater difficulty, or much slower, in *stewed Apples* than in *pure Water*; and as stewed Apples are little else than water mixed with a very small proportion of fibrous and mucilaginous matter, this shows that the conducting power of water with regard to Heat *may be impaired*.

The results of the following Experiments will serve to confirm this conclusion.

		Time the Heat was passing OUT OF the Thermometer,			
		Through stewed Apples.		Through Water.	
		Exp. No. 2.	Exp. No. 4.	Exp. No. 6.	Exp. No. 8.
		Seconds.	Seconds.	Seconds.	Seconds.
In cooling the Thermometer from the temperature of 200° to that of - 180° from 180° to 160° 160° to 140° 120° 100° 80° 60° 40°	}	80	74	46	37
		75	72	42	37
		84	83	43	43
		107	101	54	51
		141	136	73	73
		198	190	112	105
		321	307	200	204
		775	733	483	461
Total time in cooling from 200° to 40°	}	1781	1696	1053	1011
Times employed in cooling the instrument 80 degrees, viz. from 160° to 80°	}	530"	510"	282"	272"
Mean time in cooling it from 160° to 80°	}	In stewed Apples 520"		In Water 277"	

As the heating or cooling of the instrument goes on very slowly when it approaches to the temperature of the medium in which it is placed, while, on the other hand, this process is very rapid when, the temperature of the instrument being very different from that of the medium, it is first plunged into it, both these circumstances conspire to render the observations made at the extremities of the scale of the thermometer more subject to error, and consequently less satisfactory than those made nearer the middle of it: In order that the general conclusions

conclusions drawn from the result of the Experiments might not be vitiated by the effects produced by these unavoidable inaccuracies, instead of estimating the celerity of the passage of the Heat by the times elapsed in heating and cooling the thermometer *through the whole length of its scale*, or between the point of freezing to that of boiling water, I have taken the times elapsed in heating and cooling it *80 degrees in the middle of the scale*, viz. between 80° and 160° , as the measure of the conducting powers of the substances through which the Heat was made to pass.

I have, however, noted the times which elapsed in heating and cooling the instrument through a much larger interval, namely, through an interval of 168 degrees in *heating*,—or from 32° to 200° ,—and in *cooling* through 160 degrees, or from 200° to 40° .

In respect to the *cooling* of the instrument, it is necessary that I should inform my reader, that though I have not in the Tables of the Experiments mentioned any higher temperature than that of 200° , yet the instrument was always heated to the point of boiling water, which, under the pressure of the atmosphere at Munich, where the Experiments were made, was commonly about $209\frac{1}{2}$ deg. of Fahrenheit's scale. The instrument being kept in boiling water till its thermometer appeared to be quite stationary, was then taken out of the water, and instantly plunged into melting ice, and the time was observed and carefully noted down when the liquid

in its thermometer passed the division of its scale which indicated 200° , as also when it arrived at the other divisions indicated in the Tables.

With regard to the four last mentioned Experiments, (No. 2, 4, 6, and 8,) it will be found on examination that their results correspond very exactly with those before described; and they certainly prove in a very decisive manner this important fact:—*that a small proportion of certain substances, on being mixed with water, tend very powerfully to impair the conducting power of that Fluid in regard to Heat.*

In the Experiments No. 1 and No. 2, which were both made on the same day, and in the order in which they are numbered, the Heat was considerably more obstructed in its passage through the mass of stewed Apples which surrounded the thermometer than in the Experiments No. 3 and No. 4, which were made on the following day. It is probable that this was occasioned by some change in the consistency of this soft mass of the stewed Apples which had taken place while the instrument was left to repose in the interval between the Experiments: but, instead of stopping to show how this might be explained, I shall proceed to give an account of some Experiments from the results of which we shall derive information that will be much more satisfactory than any speculations I could offer on that subject.

Supposing Heat to be propagated in water in the same manner as it is propagated in air and other elastic

elastic Fluids, namely, that it is *transported* by its particles, these particles being put in motion by the change which is produced in their specific gravity, by the change of temperature, and that there is no communication whatever, or *interchange of their Heat*, among the particles of *the same Fluid*,—in that case, it is evident that the Propagation of Heat in a Fluid may be obstructed in two ways, namely, by diminishing its *Fluidity*, (which may be done by dissolving in it any mucilaginous substance,) or, more simply, by merely embarrassing and obstructing the motion of its particles in the operation of transporting the Heat,—which may be effected by mixing with the Fluid any solid substance,—(it must be a non-conductor of Heat however,)—in small masses, or which has a very large surface in proportion to its solidity.

In the foregoing Experiments with *stewed Apples*, the passage of the Heat in the water, (which constituted by far the greatest part of the mass,) was doubtless obstructed in both these ways. The mucilaginous parts of the Apples diminished very much the fluidity of the water, at the same time that the fibrous parts served to embarrass its internal motions.

In order to discover the *comparative effects* of these two causes, it was necessary to separate them; or to contrive Experiments, in which only one of them should be permitted to act at the same time. This I endeavoured to do in the following manner.

To ascertain the effects produced by diminishing the *fluidity* of water, I mixed with it a small quantity of *starch*, namely, 192 grains in weight to 2276 grains of water;—and to determine the effects produced by merely *embarrassing* the water in its motions, I mixed with it an equal proportion (by weight) of EIDERDOWN. The starch was boiled with the water with which it was mixed; as was also the eiderdown. This last-mentioned substance was boiled in the water, in order to free it from air, which, as is well known, adheres to it with great obstinacy.

In order that these Experiments might, with greater facility, be compared with those which were made with *stewed Apples*, and with *pure Water*, I shall place their results all together, in the following Tables.

Time the Heat was in passing INTO the Thermometer,			
Through a Mixture of 2276 grains of Water, and 192 grs. of STARCH.	Through a Mixture of 2276 grains of Water, and 192 grs. of EIDER-DOWN.	Through STEWED APPLES.	Through pure WATER.
Experiment No. 9.	Experiment No. 11.	Mean of two Exp. N° 1 & N° 3.	Mean of two Exp. N° 5 & N° 7.
Seconds.	Seconds.	Seconds.	Seconds.
101	83	92	45
72	55	71	35½
64	49	58½	32½
63	52	62½	30
74	57	69½	36½
89	67	86	44
115	93	117	61½
178	133	179	91½
453	360	362	220½
1109	949	1096½	597
341"	269"	335"	172"

In heating the Thermometer }
 from 32° to 40° }
 from 40° to 60° }
 60° to 80° }
 100° }
 120° }
 140° }
 160° }
 180° }
 200° }

Total times }
 in heating the }
 instrum^t from }
 32° to 200° }
 Times employ- }
 ed in heating }
 the Thermo- }
 meter 80 de- }
 grees, viz. from }
 80° to 160° }

Time the Heat was in passing OUT of the Thermometer,				
	Through a Mixture of 2276 grains of Water, & 192 grs. of STARCH.	Through a Mixture of 2276 grains of Water, and 192 grs. of EIDER- DOWN.	Through STEWED APPLES.	Through pure WATER.
	Exp. No. 10.	Experiment No. 12.	Mean of two Exp. N ^o 2 & N ^o 4.	Mean of two Exp. N ^o 6 & N ^o 8.
	Seconds.	Seconds.	Seconds.	Seconds.
In cooling the Thermometer from 200° to 188°	69	68	77	41½
from 180 to 160°	66	61	73½	39½
160° to 140°	74	72	83½	43
120°	92	91	104	52½
100°	119	120	138½	73
80°	173	177	194	108½
60°	283	279	314	202
40°	672	673	754	472
Total times in cooling from 200 to 40°	1548	1541	1749½	1032
Times employ- ed in cooling the instrument 80 degrees, viz. from 160° to 80°	468"	460"	520"	277"

As the results of these Experiments prove, in the most decisive manner, that the Propagation of Heat in water is retarded, not only by those things which diminish its fluidity, but also by those which, by mechanical means, and without forming any combination with it whatever, merely obstruct its internal motions; it appears to me, that this proves, almost to a

demonstration, that Heat is propagated in water *in consequence* of its internal motions;—or that it is transported or *carried* by the particles of that liquid, and that it does not spread and expand in it as has generally been imagined.

I have shewn in another place, and I believe I may venture to say I have proved *, that Heat is actually propagated in *air*, in the same manner I here suppose it to be propagated in water; and if the conducting powers of both these fluids are found to be impaired by the *same means*, it affords very strong grounds to conclude that they both conduct Heat in the *same manner*: but this has been found to be actually the case.

Eider-down, which cannot affect the specific qualities of either of those fluids, and which certainly does no more when mixed with them, than merely to obstruct and embarrass their internal motions, has been found to retard very much the Propagation of Heat in both of them: on comparing these Experiments with those I formerly made on the conducting power of air, it will even be found, that the conducting power of water is nearly, if not quite, as much impaired by a mixture of *eider-down* as that of air.

In the course of my Experiments on the various substances used in forming artificial clothing for confining Heat, I found that the thickness of a stratum of air, which served as a barrier to Heat, remaining the same, the passage of Heat through it was sometimes rendered more difficult by in-

* See Philosophical Transactions, 1792.

creasing the quantity of the light substance which was mixed with it to obstruct its internal motion.

To see if similar effects would be produced by the same means when Heat is made to pass through water, I repeated the Experiments with *eider-down*, reducing the quantity of it mixed with the water to 48 grains, or *one quarter* of the quantity used in the Experiments No. 11 and No. 12.

The results of these Experiments, and a comparison of them with those before mentioned, may be seen in the following Tables :

Time the Heat was in passing INTO the Thermometer,			
In heating the Ther- mometer from 32° to 40° from 40° to 60° 60° to 80° 100° 120° 140° 160° 180° 200° Total times in heating from 32° to 200° Times employed in heating the instru- ment 80 degrees, or from 80° to 160°	Through Water with 48 grs. or $\frac{1}{50}$ of its bulk of EIDER- DOWN.	Through Water with 192 grs. or $\frac{4}{50}$ of its bulk of EIDER- DOWN.	Through pure WATER.
	Experiment No. 13.	Experiment No. 11.	Mean of two Exp. N° 5 & N° 7.
	Seconds.	Seconds.	Seconds.
	51	83	45
	47	55	$35\frac{1}{2}$
	39	49	$32\frac{1}{2}$
	40	52	30
	45	57	$36\frac{1}{2}$
	56	67	44
	74	93	$61\frac{1}{2}$
	118	133	$91\frac{1}{2}$
	293	360	$220\frac{1}{2}$
	763	949	597
	215"	269"	172"

Time the Heat was passing OUT of the Thermometer,			
Through Water with 48 grs. or $\frac{1}{30}$ of its bulk of EIDER-DOWN.		Through Water with 192 grs. or $\frac{1}{15}$ of its bulk of EIDER-DOWN.	Through pure WATER.
Experiment No. 14.	Experiment No. 12.	Mean of two Exp. N ^o 6 & N ^o 8.	
Seconds.	Seconds.	Seconds.	
In cooling the Thermometer from 200° to 180°	49	68	41 $\frac{1}{2}$
from 180° to 160°	50	61	39 $\frac{1}{2}$
160° to 140°	56	72	43
120°	70	91	52 $\frac{1}{2}$
100°	96	120	73
80°	151	177	108 $\frac{1}{2}$
60°	262	279	202
40°	661	673	472
Total times in cooling from 200° to 40°	1395	1541	1032
Times employed in cooling the instrument 80 degrees, viz. from 160° to 80°	373"	460"	277"

The results of these Experiments are extremely interesting. They not only make us acquainted with a new and very curious fact,—namely, that feathers, and other like substances, which, in air, are known to form very warm covering for confining Heat, not only serve the same purpose in water, but that their effects in preventing the passage of Heat is even greater in water than in air.

This discovery, if I do not deceive myself, throws a very broad light over some of the most interesting parts of the economy of Nature, and gives us much satisfactory information respecting the final causes of many phænomena which have hitherto been little understood.

As *liquid water* is the vehicle of Heat and nourishment, and consequently of life, in every living thing; and as water, left to itself, freezes with a degree of cold much less than that which frequently prevails in cold climates, it is agreeable to the ideas we have of the wisdom of the Creator of the world, to expect that effectual measures would be taken to preserve a sufficient quantity of that liquid in its fluid state, to maintain life during the cold season: and this we find has actually been done; for both plants and animals are found to survive the longest and more severe winters; but the means which have been employed to produce this miraculous effect have not been investigated;—at least not, in as far as they relate to vegetables.

But as animal and vegetable bodies are essentially different in many respects, it is very natural to suppose, that the means would be different which are employed to preserve them against the fatal effects which would be produced in each by the congelation of their fluids.

Among organized bodies, which live on the surface of the earth, and which of course are exposed to the vicissitudes of the seasons, we find that as the proportion of fluids to solids is greater, the

the greater is the Heat which is required for the support of life and health; and the less are they able to endure any considerable change of their temperature.

The proportion of Fluids to Solids is much greater in *animals* than in vegetables; and in order to preserve in them the great quantity of Heat which is necessary to the preservation of life, they are furnished with lungs, and are warmed by a process similar to that by which Heat is generated in the combustion of inflammable bodies.

Among *vegetables*, those which are the most succulent are *annual*. Not being furnished with lungs to keep the great mass of liquids warm, which fill their large and slender vessels, they live only while the genial influence of the sun warms them, and animates their feeble powers; and they droop and die as soon as they are deprived of his support.

There are many tender plants to be found in cold countries, which die in the autumn, the roots of which remain alive during the winter, and send off fresh shoots in the ensuing spring. In these we shall constantly find the roots more compact and dense than the stalk, or with smaller vessels, and a smaller proportion of Fluids.

Among the trees of the forest, we shall constantly find, that those which contain a great proportion of *thin watery liquids*, not only shed their leaves every autumn, but are sometimes frozen, and actually killed, in severe frosts. Many thousands of the largest walnut-trees were
killed

killed by the frost in the Palatinate, during the very cold winter in the year 1788; and it is well known that few, if any, of the deciduous plants of our temperate climate would be able to support the excessive cold of the frigid zone.

The trees which grow in those inhospitable climates, and which brave the colds of the severest winters, contain very little watery liquids. The sap which circulates in their vessels is thick and viscous, and can hardly be said to be fluid. Is there not the strongest reason to think, that this was so contrived for the express purpose of preventing their being deprived of all their Heat, and killed by the cold during the winter?

We have seen by the foregoing Experiments, how much the Propagation of Heat in a liquid is retarded by diminishing its fluidity; and who knows but this may continue to be the case, as long as any degree of fluidity remains?

As the bodies and branches of trees are not covered in winter by the snow which protects their roots from the cold atmosphere, it is evident that extraordinary measures were necessary to prevent their being frozen. The bark of all such trees as are designed by nature to support great degrees of cold, forms a very warm covering; but this precaution alone would certainly not have been sufficient for their protection. The sap, in all trees which are capable of supporting a long continuance of frost, grows thick and viscous on the approach the winter. What more important purpose could this change answer, than that here indicated?—

And

And it would be more than folly to pretend that it answers no useful purpose at all.

We have seen, by the results of the foregoing Experiments, how much the simple embarrassment of liquids in their internal motions tends to retard the Propagation of Heat in them, and consequently its passage out of them;—and when we consider the extreme smallness of the vessels in which the sap moves in vegetables, and particularly in large trees;—when we recollect that the substance of which these small tubes are formed, is one of the best non-conductors of Heat known*;—and when we advert to the additional embarrassments to the passage of the Heat, which arise from the increased viscosity of the sap in winter, and to the almost impenetrable covering for confining Heat, which is formed by the bark, we shall no longer be at a loss to account for the preservation of trees during the winter, notwithstanding the long continuation of the hard frosts to which they are annually exposed.

* I lately, by accident, had occasion to observe a very striking proof of the extreme difficulty with which Heat passes in wood. Being present at the foundery at Munich, when cannons were casting, I observed that the founder used a wooden instrument for stirring the melted metal. It was a piece of oak plank, green, or unseasoned, about ten inches square and two inches thick, with a long wooden handle, which was fitted into a hole in the middle of it. As this instrument was frequently used, and sometimes remained a considerable time in the furnace, in which the Heat was most intense, I was surprized to find that it was not consumed; but I was still more surprized, on examining the part of the plank which had been immersed in the melted metal, to find that the Heat had penetrated it to so inconsiderable a depth, that, at the distance of one-twentieth of an inch below its surface, the wood did not seem to have been in the least affected by it. The colour of the wood remained unchanged, and it did not appear to have lost even its moisture.

On the same principles we may, I think, account, in a satisfactory manner, for the preservation of several kinds of fruit,—such as apples and pears for instance,—which are known to support, without freezing, a degree of cold, which would soon reduce an equal volume of *pure water* to a solid mass of ice.

At the same time that the compact skin of the fruit effectually prevents the evaporation of its fluid parts, which, as is well known, could not take place without occasioning a very great loss of Heat, the internal motions of those fluids are so much obstructed by the thin partitions of the innumerable small cells in which they are confined, that the communication of their Heat to the air ought, according to our hypothesis, to be extremely slow and difficult. These fruits do, however, freeze at last, when the cold is very intense; but it must be remembered, that they are composed almost entirely of liquids, and of such liquids as do not grow viscous with cold; and moreover, that they were evidently not designed to support, for a long time, very severe frosts.

Parsnips and carrots, and several other kinds of roots, support cold without freezing, still longer than apples and pears, but these are less watery, and I believe the vessels in which their fluids are contained, are smaller; and both these circumstances ought, according to our assumed principles, to render the passage of their Heat out of them more difficult, and consequently to retard their congelation.

But

But there is still another circumstance, and a very remarkable one indeed, which, if our conjectures respecting the manner in which Heat is propagated in liquids be true, must act a most important part in the preservation of Heat, and consequently of animal and vegetable life, in cold climates. But as the probability of all these deductions must depend, very much, on the evidence which is brought to prove the great fundamental fact on which they are established;—that respecting the internal motions among the particles of liquids, which *necessarily* take place when they are heated or cooled;—before I proceed any farther in these speculations, I shall endeavour to throw some more light on that curious and interesting subject,

C H A P. II.

Farther Investigations of the internal Motions among the Particles of Liquids, which necessarily take place when they are heated or cooled.—Description of a mechanical Contrivance, by which these Motions in Water were rendered visible.—An Account of various amusing Experiments, which were made with this new-invented Instrument.—They lead to an important Discovery.—Heat cannot be propagated DOWNWARDS in Liquids, as long as they continue to be condensed by Cold.—Ice found, by Experiment, to melt more than eighty times slower, when boiling hot Water stood on its Surface, than when the Ice was suffered to swim on the Surface of the hot Water.—The melting of Ice by Water standing on its Surface can be accounted for, even on the Supposition that Water is a perfect Non-conductor of Heat.—According to the assumed Hypothesis, Water only eight Degrees of Fahrenheit's Scale above the freezing Point, or at the Temperature of 40° , ought to melt as much Ice, in any given Time, when standing on its Surface, as an equal Volume of that Fluid, at any higher Temperature, even were it boiling hot.—This remarkable Fact is proved by a great Variety of decisive Experiments.—Water at the Temperature of 41° is found to melt even MORE Ice, when standing on its Surface, than boiling-hot Water.—The
Results

Results of all these Experiments tend to prove that Water is, in fact, a perfect Non-conductor of Heat ; or that Heat is propagated in it, merely in consequence of the Motions it occasions among the insulated or solitary Particles of that Fluid, which, among themselves, have no Communication or Inter-course whatever in this Operation. — The Discovery of this Fact opens to our View one of the grandest and most interesting Scenes in the Economy of Nature.

As the particles of water, as also of all other Fluids, are infinitely too small to be seen by human eyes, their motions must of course be imperceptible by us ; but we are frequently enabled to judge, with the utmost certainty, of the motions of invisible Fluids, by the motions they occasion in visible bodies. Air is an invisible Fluid, but we acquire very just notions of the motions in air, by the dust, and other light bodies which are carried along with it in its motions. Nobody who has ever seen a whirlwind sweep over the surface of a ploughed field in dry weather, can have any doubt respecting the nature of the motions, into which the air is thrown on those occasions ; notwithstanding that they are extremely complicated, and would be very difficult to describe.

It was by the motions of the very fine particles of dust, which by accident had been mixed with the spirits of wine in my large thermometer, and which, when strongly illuminated by the direct beams of the sun, became visible, that I first dis-

covered the internal motions in that Fluid, which take place when it is cooling ; and, availing myself of this kind hint, I contrived to render the internal motions of water equally visible. This, I immediately saw, could be done with the utmost facility, if I could but find any solid body of the same specific gravity as water, which would be proper to mix with it ;—that is to say, that would not be liable to be dissolved by it, or to be reduced to such small particles as to become itself invisible ; but such a substance was not to be found. On reflection it occurred to me, that it is very fortunate that such substances do not abound ; for otherwise we should find great difficulty in procuring water in a pure state.

Not being able to find any solid substance fit for my purpose, of the same specific gravity as pure water, I was obliged to have recourse to the following stratagem.

Looking over the tables of specific gravities, I found that the specific gravity of transparent yellow amber was but a little greater than that of water, being 1.078, while that of water is 1.000 ; and it occurred to me, that by dissolving a certain quantity of pure alkaline salt, I might augment its specific gravity, or rather bring the specific gravity of the solution to be precisely equal to that of the amber, without impairing the transparency of the liquid, or changing any of its properties, by which the manner of its receiving and transporting Heat could be sensibly affected.

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This contrivance was put in execution in the following manner, with complete success. Having provided myself with a number of glass globes of various sizes, with long cylindrical necks, I chose one which was about two inches in diameter, with a cylindrical neck $\frac{1}{4}$ of an inch in diameter, and twelve inches long; and putting into it about half a tea-spoonful of yellow amber, in the form of a coarse powder, (the pieces, which were irregular in their forms, and transparent, being about the size of mustard-seeds,) poured about it a certain quantity of distilled water, which was at the temperature of the air in my room,—(about 60° F.)

Finding, as I expected, that the amber remained at the bottom of the globe, I now added to the water as much of a saturated solution of pure vegetable alkali, as was sufficient to increase the specific gravity of the water, (or rather of the diluted saline solution,) till the pieces of amber began to float, and remained apparently motionless, in any part of the liquid where they happened to rest.

As the glass body was not yet as full as I wished, I continued to add more of the alkaline solution, and of water, in due proportions, till the globe was full; and also till its cylindrical tube was filled to within about three inches of its end; and then closed it well with a clean cork.

Having shaken the contents of this glass body well together, I placed it, with its cylindrical tube in a vertical position, on a wooden stand, and left it to repose in quiet, in order to see how long the solid particles of amber—(which appeared to be very

equally dispersed about in the whole mass of the liquid)—would remain suspended.

Though the greater number of these particles seemed at first to have no tendency either to ascend or to descend, yet some of them soon began to move very slowly upwards, and others to move as slowly downwards; and as these particles were moving at the same time promiscuously in all parts of the same liquid, and even in the same part of it in both directions at the same time, the ascending and descending particles frequently passing each other so near as to touch, I saw that these motions were independent of any internal motion of the liquid, and arose merely from the difference of the specific gravity of the different small pieces of the amber, and of that of the liquid. Some of the pieces of amber being evidently heavier than the liquid, moved downward, while other which were lighter, ascended to its surface.

Finding that there was so much difference in the specific gravities of the different pieces of amber, I now added more of this substance to the liquid, and suffering it to subside after I had shaken it well together, I gently poured off what had risen to the top of the liquid, and retaining only that which had settled at the bottom of it, I increased the specific gravity of the liquid by adding a little of the alkaline solution, till the small pieces of amber which remained in the glass were just buoyed up and suspended in the different parts of the Fluid, where they seemed to have taken their permanent stations.

I had

I had now an instrument which appeared to me to be well calculated for the very interesting Experiments I had projected, and it will easily be imagined that I lost no time in making use of it.

The first Experiment I made with this instrument was to plunge it into a tall glass jar, nearly filled with water almost boiling hot. The result was just what I expected. Two currents, in opposite directions, began at the same instant to move with great celerity in the liquid in the cylindrical tube, the ascending current occupying the sides of the tube, while that which moved downwards occupied its axis.

As the saline liquor grew warm, the velocity of these currents gradually diminished; and at length, when the liquor had acquired the temperature of the surrounding water in the jar, these motions ceased entirely.

On taking the glass body out of the hot water, the internal motions of the liquor recommenced; but the currents had changed their directions, that which occupied the axis of the tube being now the ascending current.

When the cylindrical tube, instead of being held in a vertical position, was inclined a little, the ascending current occupied that side of it which happened to be uppermost, while the under side of it was occupied by the current which moved (with equal velocity) downwards.

When the contents of the glass body had acquired the temperature of the air of the room,
these

these motions ceased, but they immediately recommenced on exposing the instrument to any change of temperature.

In all cases where the instrument *received Heat*, the current in the axis of its cylindrical tube, when it was placed in a vertical position, (and that which occupied its *upper side* when it was inclined,) moved downwards.—When it parted with Heat its motion was in an opposite direction, that is to say, *upwards*.

A change of temperature amounting only to a few degrees of Fahrenheit's scale, was sufficient to set the contents of the instrument in motion; and the motion was more or less rapid as the velocity was greater or less with which it acquired or parted with Heat, and the motion was most rapid in those parts of the instrument where the communication was not rapid.

A partial motion might at any time be produced in any part of the instrument by applying to that part of it any body either hotter or colder than the instrument. If the body so applied were hotter than the instrument, the motion of the saline liquor in it in that part of it immediately in contact with the hot body, was *upwards*,—if colder, downwards; and whenever a hot or cold body produced a current upwards or downwards, this current immediately produced another in some other part of the liquid which flowed in an opposite direction.

On inclining the cylindrical tube of the instrument an angle of about 45 degrees with the plane of the horizon, and holding the middle of it over the
flame

flame of a candle, at the distance of three or four inches above the point of the flame; the motion of the Fluid in the upper part of the tube became excessively rapid, while that in the lower end of it where it was united to the globe, as well as that in the globe itself, remained almost perfectly at rest.

I even found that I could make the Fluid in the upper part of the tube *actually boil*, without that in the lower part of it appearing to the hand to be sensibly warmed. But when the flame was directed against the lower part of the tube, all the upper parts of it in contact with the liquid, and especially that side of it which was uppermost as it lay in an inclined position, where the ascending current was most rapid, where it impinged against the glass, were very soon heated very hot.

The motions in opposite directions, in the liquid in the tube, were exceedingly rapid on this sudden application of a strong Heat, and afforded a very entertaining sight:—but to a scientific observer they were much more than amusing. They detected Nature, as it were, in the very act, in one of her most hidden operations, and rendered motions visible in the midst of an invisible medium which never had been seen before, and which most probably had never been suspected.

Encouraged by this success, and confirmed in my opinions respecting the interesting fact I had undertaken to investigate, I now proceeded with confidence to still more direct and decisive Experiments.

It

It is an opinion which, I believe, is generally received among philosophers, that water cannot be heated in contact with ice: reflecting on the subject, I immediately perceived that either this must be a mistake, or all my ideas respecting the manner in which Heat is propagated in that Fluid must be erroneous. I saw that as long as the ice floats at the surface of water which is attempted to be warmed over a fire, (or in any other way,) the ice-cold water which results from the melting of the ice, must, according to my own hypothesis, descend, and spreading over the bottom of the containing vessel, and, before it has time to be much heated, being in its turn forced to give place to the ice-cold water which, as long as any ice remains, continues to descend in an uninterrupted stream as long as this operation is going on, the mass of the water cannot be much heated; but on the supposition that water is not a conductor of Heat, according to the common acceptance of that term, or that Heat cannot pass in that Fluid except when it is *carried* by its particles, which, being put in motion by the change it occasions in their specific gravity, *transports* it from place to place, it does not appear how ice, if instead of being permitted to swim on water, were confined at the bottom of it, or at any given distance below its surface, could in any way affect the temperature of the superincumbent water, or prevent its receiving Heat from other bodies.

Were water a conductor of Heat, there is no doubt but that the influence of the presence of the ice

ice would be propagated in the water in all directions.

The metals are all conductors of Heat, and Professor PIETET found by an ingenious and decisive Experiment *, that in a bar of copper 33 inches in length, placed in a vertical position, Heat passed downwards as well as upwards, and nearly with the same facility in both these directions; and if it can be shown that Heat cannot descend in water, that alone will, I imagine, be thought quite sufficient to prove that water is not a conductor of Heat.

When we meditate profoundly on the nature of Fluidity, it seems to me that we can perceive some faint lights which might lead us to suspect that the *cause*, and I may say the very *essence of fluidity*, is that property which the particles of bodies acquire when they become fluid, by which all farther interchange or communication of Heat among them is prevented. But however this may be, the result of the following Experiments will certainly be considered as affording indisputable evidence of one important fact respecting the manner in which Heat is propagated in water.

Experiment, No. 15.

Into a cylindrical glass jar 4.7 inches in diameter, and 14 inches high, I fitted a circular cake of ice nearly as large as the internal diameter of the jar, and $3\frac{1}{2}$ inches thick, weighing $10\frac{1}{2}$ oz.

* *Essais de Physique*, tome 1. Genève 1790.

This cake of ice being ready, I now poured into the jar 6 lb. $1\frac{1}{4}$ oz. Troy, of boiling-hot water, and putting the ice gently into it, I found that it was entirely melted in 2 minutes and 58 seconds.

Having found by this Experiment how long the ice was in melting at the surface of the hot water, I now endeavoured to find out whether it would not require a longer time to melt at the bottom of the water.

Experiment, No. 16.

Into the same jar which was used in the foregoing Experiment, I now put a cake of ice of the same form and dimensions as that above described, but instead of letting it swim at the surface of the hot water, I fastened it down on the bottom of the jar, and poured the water upon it.

This cake of ice was fastened down in the jar by means of two slender and elastic pieces of deal about $\frac{1}{8}$ of an inch thick, and $\frac{1}{4}$ of an inch wide, which being a trifle longer than the internal diameter of the jar, were of course a little bent when they were introduced into it in an horizontal position, and on being put down upon the ice, at right angles to each other, served to confine the ice, and prevent its rising up to the surface when the water was put into the jar upon it.

To protect the ice while the boiling-hot water was pouring into the jar, its surface was covered with a circular piece of strong writing paper, which was afterwards removed as gently as possible by means

means of a string which was fastened to one side of it; and to prevent the glass jar from being cracked by the sudden application of the boiling-hot water, I began by pouring a small quantity of cold water into the jar,—just enough to fill up the interstices between the ice and the glass, and to cover the ice to the height of about $\frac{1}{4}$ of an inch; and in pouring the hot water into the jar, out of a large tea-kettle in which it had been boiled, I took care to direct the stream against the middle of the circular piece of paper which covered the ice.

The jar with the ice and the hot water in it being placed on a table near a window, I drew away as gently as possible the paper which covered the surface of the ice, and prepared myself to observe at my ease the result of this most interesting Experiment.

A very few moments were sufficient to show me that my expectation with regard to it would not be disappointed. In the former Experiment a similar cake of ice had been entirely melted in less than three minutes; but in this, after more than twice that time had elapsed, the ice did not show any apparent signs of even *beginning to melt*. Its surface remained smooth and shining, and the water immediately in contact with it appeared to be perfectly at rest, though the internal motions of the hot water above it, which was giving off its Heat to the sides of the jar and to the air, were very rapid, as I could distinctly perceive by means of some earthy particles

or other impurities which this water happened to contain.

I examined the ice with a very good lens, but it was a long time before I could perceive any signs of its melting. The edges of the cake remained sharp, and the minute particles of dust, which by degrees were precipitated by the hot water as it grew colder, remained motionless as soon as they touched the surface of the ice.

As the hot water had been brought from the kitchen in a tea-kettle, it was not quite boiling hot when it was poured into the jar. After it had been in the jar one minute, I plunged a thermometer into it, and found its temperature to be at 180° .

After 12 minutes had elapsed, its temperature at the depth of one inch under the surface was 170° . At the depth of seven inches, or one inch above the surface of the ice, it was at $169\frac{1}{8}$; while at only $\frac{3}{4}$ of an inch lower, or $\frac{1}{4}$ above the surface of the ice, its temperature was 40° .

When 20 minutes had elapsed, the Heat in the water at different depths was found to be as follows:

Immediately above the surface of the ice	40°
At the distance of $\frac{1}{2}$ an inch above it	46°
At 1 inch	130°
At 3 inches	159°
At 7 inches	160°

When 35 minutes had elapsed, the Heat was as follows :

At the surface of the ice	40°
$\frac{1}{2}$ an inch above it	76°
5	1 inch

1 inch above it	110°
2 inches	144°
3 inches	148°
5 inches	148½°
7 inches	149°

At the end of one hour the Heat was as follows:

At the surface of the ice	40°
1 inch above it	80°
2 inches	118°
3 inches	128°
4 inches	130°
7 inches	131°

After 1 hour and 15 minutes had elapsed, the Heat was found to be as follows:

At the surface of the ice	40°
1 inch above it	82°
2 inches	106°
3 inches	123°

The Heat of the water had hitherto been taken near the side of the jar;—in the two following trials it was measured in the middle or axis of the jar.

When 1 hour and 30 minutes (reckoning always from the time when the boiling-hot water was poured into the jar) had elapsed, the Heat of the water in the middle of the jar was found to be as follows:

At the surface of the ice	40°
1 inch above it	84°
2 inches	115°
3 inches	116°
7 inches	117°

When 2 hours had elapsed, the Heat in the middle of the jar was found to be as follows :

At the surface of the ice	-	-	40°
1 inch above it	-	-	76°
2 inches	-	-	94°
3 inches	-	-	106°
4 inches	-	-	108°
6 inches	-	-	108½°
7 inches	-	-	108½°

An end being now put to the Experiment, the hot water was poured off from the ice, and on weighing that which remained, it was found that 5 oz. 6 grains Troy (=2406 grains) of ice had been melted.

Taking the mean temperature of the water at the end of the Experiment at 106°, it appears that the mass of hot water (which weighed $73\frac{1}{4}$ ounces) was cooled 78 degrees, or from the temperature of 184° to that of 106° during the Experiment. Now, as it is known that one ounce of ice absorbs just as much Heat in being changed to water as one ounce of water loses in being cooled 140 degrees, it is evident that one ounce of water which is cooled 78 degrees, gives off as much Heat as would be sufficient to melt $\frac{78}{140}$ of an ounce of ice ; consequently the $73\frac{1}{4}$ ounces of hot water, which in this Experiment were cooled 78 degrees, actually gave off as much Heat as would have been sufficient to have melted $\frac{73\frac{1}{4} \times 78}{140} = 40\frac{5}{16}$ ounces of ice.

But the quantity of ice actually melted was only about 5 ounces ; and hence it appears that
less

less than one-eighth part of the Heat lost by the water was communicated to the ice; the rest being carried off by the air.

As the same quantity of hot water was used in this Experiment, and in that, No. 15, which immediately preceded it, and as this water was contained by the same vessel,—(the glass jar above described,)—it appears that ice melts more than *eighty times slower* at the bottom of a mass of boiling-hot water, than when it is suffered to swim on its surface: For, as in the Experiment No. 15, $10\frac{1}{4}$ oz. of ice were melted in 2 minutes and 58 seconds, 5 ounces at least must have been melted in 1 minute and 29 seconds; but in the Experiment No. 16, 2 hours or 120 minutes were employed in melting 5 ounces.

The ice however *was melted*, though very slowly, at the bottom of the hot water, and that circumstance alone would have been sufficient to have overturned my hypothesis respecting the manner in which Heat is propagated in liquids; had I not found means to account in a satisfactory manner for that fact, without being obliged to abandon my former opinions.

In about half an hour after the hot water had been poured into the jar, in the last Experiment, examining the surface of the ice I discovered an appearance which fixed my attention and excited all my curiosity: I perceived that the ice had been melted and diminished at its surface, excepting only where it had been covered, or as it were *shadowed*
by

by the flat slips of deal by which the cake of ice was fastened down in its place.

Had the ice been protected and prevented from being melted by that piece of the wood *only*, which, being undermost of the two, reposed immediately on the surface of the ice, I should not perhaps have been much surpris'd ; but that part of the surface of the ice being likewise protected which was situated immediately under the other piece of wood,—that which, lying across the under piece, and resting on it, *did not touch the ice any where except just at its edge*, that circumstance attracted my attention, and I could at first see no way of accounting for these appearances but by supposing that the ice had been melted by the *calorific rays* which had been emitted by the hot water ; and that those parts of the ice which had been *shadowed* by the pieces of deal receiving none of these rays, had of course not been melted.

I was so much struck with these appearances, that I immediately made the following Experiments, with a view merely to the elucidation of this matter.

Experiment, No. 17.

Into a cylindrical glass jar, $6\frac{1}{2}$ inches in diameter, and 8 inches high, I put a circular cake of ice, as long as could be made to enter the jar, and about $3\frac{1}{2}$ inches thick ; and on the flat and even surface of the ice I placed a circular plate of the thinnest tin I could procure, near $6\frac{1}{2}$ inches in diameter,

meter, or sufficiently large just to cover the ice. This plate of tin (which, to preserve its form, or keep it quite flat, was strengthened by a strong wire, which went round it at its circumference) had a circular hole in its centre, just two inches in diameter, and it was firmly fixed down on the upper surface of the cake of ice, by means of several thin wooden wedges which passed between its circumference and the sides of the jar.

A second circular plate of tin, with a circular hole in its centre two inches in diameter, and in all other respects exactly like that already described, was now placed over the first, and parallel to it, at the distance of just one inch, and like the first was firmly fixed in its place by wooden wedges.

These perforated circular plates being fixed in their places, the jar was placed in a room where Fahrenheit's thermometer stood at 34° ; and ice-cold water was poured into it till the water just covered the upper plate; and then the jar was filled to within half an inch of its brim with boiling water; and being covered over with a board, was suffered to remain quite two hours.

At the end of this time, the water, which was still warm, was poured off, and the circular plate being removed, the ice was examined.

A circular excavation, just as large as the hole in the tin plate which covered the ice, (namely two inches in diameter,) and corresponding with it,—perfectly well defined, and about $\frac{2}{16}$ of an inch deep in the centre, had been made in the ice.

This was what I expected to find; but there was something more, which I did not expect, and which, for some time, I was quite at a loss to account for. Every part of the surface of the ice which had been covered by the tin plate, appeared to be perfect, level, and smooth, and showed no signs of its having been melted or diminished, excepting only in one place, where a channel, about an inch wide, and a little more than $\frac{1}{16}$ of an inch deep, which showed evident marks of having been formed by a stream of warm water, led from the excavation just mentioned, in the centre of the upper part of the cake of ice, to its circumference. As the edge, or vertical side, of the cake of ice was evidently worn away where this stream passed, there could be no doubt with respect to its direction. It certainly run *out* of the circular excavation in the middle of the ice; and though it might at first appear difficult to explain the fact, and to show how this hot water could arrive at that place, yet it was quite evident that the immediate cause of the motion of this stream of water could be no other than its specific gravity being greater than that of the rest of the water at the same depth: and that this greater specific gravity was at the same time accompanied by a higher degree of Heat, is evident from the deep channel which this stream had melted in the ice, while other parts of the surface of the ice, at the same level, were not melted by the water which rested on it. To elucidate this point, I made the following Experiment:

Experiment, No. 18.

Thinking it probable, that if the circular excavation in the ice, which answered to the circular hole in the middle of the tin-plate which covered the ice, and also to that in the second plate, which was placed an inch higher, had been melted by *radiant Heat*, (as it has improperly been called,) or by the calorific rays from the hot water; that, in that case, as some of these rays must probably have been reflected downwards at the surface of the water, in attempting to pass upwards into the air; I thought that by preventing this part of them from reaching the ice, which I endeavoured to do by causing them to be absorbed by a light black body, (a circular piece of deal board, covered over with black silk,) which I caused to swim on the surface of the water, their effects in melting the ice might perhaps be sensibly diminished. Had this really been the case, it would certainly have afforded strong grounds to suspect that these rays were in fact the cause of the appearances in question; but on making the Experiment with the greatest care, I could not perceive that the covering of the surface of the hot water with a black body produced any difference whatever in the result of the Experiment as it was first made, (Experiment No. 17,) or when this black covering was not used.

After some meditation on the subject, it occurred to me that this melting of the ice at its

upper surface could be accounted for, in a manner which appeared to me to be perfectly satisfactory; without supposing either that water is a conductor of Heat, or that the effect in question was produced by calorific rays.

Though it is one of the most general laws of nature with which we are acquainted, that all bodies, solids as well as fluids, are condensed by cold; yet, in regard to *water*, there appears to be a very remarkable exception to this law. Water, like all other known bodies, is indeed condensed by cold at every degree of temperature which is considerably higher than that of freezing, but its condensation, on parting with Heat, does not go on till it is changed to ice; but when in cooling its temperature has reached to 40 degrees of Fahrenheit's scale, or eight degrees above freezing, it ceases to be farther condensed; and on being cooled still farther, it *actually expands*, and continues to expand, as it goes on to lose more of its Heat, till at last it freezes; and at the moment when it becomes solid, and even after it has become solid, it expands still more, on growing colder. This fact, which is noticed by M. DE LUC, in his excellent treatise on the modifications of the atmosphere, has since been farther investigated and put beyond all doubt, by SIR CHARLES BLAGDEN. See Philosophical Transactions, vol. lxxviii.

Now, as water in contact with melting ice is always at the temperature of 32° , it is evident that water at that temperature must be specifically lighter than water which is eight degrees warmer,

or

or at the temperature of 40° ; consequently, if two parcels of water at these two temperatures be contained in the same vessel, that which is the coldest and lightest, must necessarily give place to that which is warmer and heavier, and currents of the warmer water will *descend* in that which is colder.

In the two last Experiments, as the circular tin-plate which covered the surface of the ice served to confine the thin sheet of water which was between the plate and the ice, as this water could not rise upwards, being hindered by the plate, and as it had no tendency to descend, it is probable that it remained in its place; and as it was *ice-cold*, it was not capable of melting the ice on which it reposed,

But as the tin-plate had a circular hole in its centre, the surface of the ice *in that part* was of course naked, and the ice-cold water in contact with it being displaced by the warmer and heavier water from above, an excavation, in the form of a shallow basin, was formed in the ice by this descending warm current.

The warm water contained in this basin overflowed its banks as soon as the basin began to be formed; and issuing out on that side which happened to be the lowest, opened itself a passage under the tin-plate to the edge of the ice, over which it was precipitated, and fell down to the bottom of the jar. The water of this rivulet being warm, it soon formed for itself a deep channel in the ice; and at the end of the Experiment it was found

found to be every where *deeper* than the bottom of the basin where it took its rise.

This manner of accounting for the appearances in question, seemed to me to be quite satisfactory; and the more I meditated on the subject, the more I was confirmed in my suspicions that *all liquids* must necessarily be perfect *non-conductors of Heat*.

On these principles I was now enabled to account for the melting of the ice at the bottom of the hot water in the Experiment No. 16; as also for the slowness with which that process went on;—and encouraged by this success, I now proceeded with confidence to plan and to execute still more decisive Experiments; from the results of which, I may venture to say it,—the important facts in question have been put beyond all possibility of doubt.

If water be in fact a perfect *non-conductor* of Heat,—that is to say, if there be *no communication whatever of Heat* between neighbouring particles or *molécules* of that fluid, (which is what I suppose,) then,—as Heat cannot be propagated in it but *only* in consequence of the motions occasioned in the fluid by the changes in the specific gravity of those particles which are occasioned by the changes of their temperature, it follows that Heat cannot be propagated *downwards* in water, as long as that fluid continues to be condensed with cold; and that it is *only in that direction*, (downwards,) that it *can be propagated* after the water has arrived at that temperature, where it begins to be expanded by cold;—which has been found to be at about the 40th degree of Fahrenheit's scale.

Reasoning

Reasoning on these principles, we are led to this remarkable conclusion; namely, that *water which is only eight degrees above the freezing point, or at the temperature of 40° , must be able to melt as much ice in any given time, WHEN STANDING ON ITS SURFACE, as an equal volume of water at any higher temperature, EVEN THOUGH IT WERE BOILING HOT.*

My philosophical reader will doubtless think that I must have had no small degree of confidence in the opinion I had formed on this interesting subject, to have had the courage to make, *even in private*, the Experiments which were necessary to ascertain that fact.

Experiment, No. 19.

Into a cylindrical glass jar, 4.7 inches in diameter, and 13.8 inches high, I put 43.87 cubic inches, or 1 lb. 11 $\frac{1}{2}$ oz. Troy, in weight, of water, and placing the jar in a freezing mixture, composed of pounded ice and common sea-salt, I caused the water to freeze into one compact mass; which adhered firmly to the bottom and sides of the jar, and which formed a cylinder of ice just three inches high.

Had the bottom of the jar been quite flat, instead of being raised or vaulted, the cylinder of ice would have been no more than 2.67 inches high.

As soon as the water in the jar was completely frozen, the jar was removed from the freezing mixture, and placed in a mixture of pounded ice and pure water, where it was suffered to remain
four

four hours, in order that the cake of ice in the jar might be brought to the temperature of 32° .

The jar still standing in a shallow dish in the pounded ice and water, the surface of which cold mixture was just on a level with the surface of the ice in the jar, I covered the top of the cake of ice with a circular piece of strong paper, and poured gently into the jar $73\frac{1}{4}$ oz. Troy of boiling-hot water, which filled to the height of eight inches above the surface of the ice. (See Plate II.)

I then removed very gently the circular piece of paper which covered the surface of the ice, and after leaving the hot water in contact with the ice a certain number of minutes, I poured it off, and—weighing immediately the jar, and the unmelted ice which remained in it,—I ascertained the *quantity of ice which had been melted* by the hot water during the time it had been suffered to remain in the jar.

This Experiment I repeated four times the same day, (16th March 1797,) varying at each repetition of it the time the water was permitted to remain on the ice. The results of these Experiments were as follows :

Number of the Experiment.	Time the hot water remained on the ice.	Temperature of the hot water when it was poured on the ice.	Temperature of the water 1 inch below its surface at the end of the Experiment.	Quantity of ice melted.
	Minutes.			Grains.
No. 19	1	186°	Not observed	1632
No. 20	$3\frac{1}{2}$	185°	Not observed	1824
No. 21	15	184°	170°	1757
No. 22	60	186°	140°	2573

From

From the results of these Experiments, it was plain that a very considerable portion of the ice which was melted, was melted in the very beginning of the Experiments, or while the hot water was actually *pouring* into the jar; which operation commonly lasted about one minute; and the irregularities in the results of the Experiments, and particularly of the three first, showed evidently that the quantity of ice melted in that operation was different in different Experiments. I had indeed foreseen that this would be the case; and on that account it was that I covered the surface of the ice with a circular piece of strong paper, and always took care to pour the water very gently into the jar: but I found that all these precautions were not sufficient to prevent very considerable anomalies in the results of the Experiments; and as I found reason to suspect that the motion in the mass of the hot water, which was unavoidably occasioned by removing the circular piece of paper which covered the ice, was the principal cause of these inaccuracies, I had recourse to another, and a better contrivance.

Having procured a flat, shallow dish, of light wood; half an inch deep, $4\frac{1}{2}$ inches in diameter, (or something less than the internal diameter of the jar,)—and about $\frac{1}{4}$ of an inch thick at its bottom, I bored a great number of very small holes through its bottom, which gave it the appearance of a sieve. This perforated wooden dish having been previously made *ice-cold*, was placed on the surface of the ice in the jar; and the hot water being

5

gently

gently poured into the dish through a long wooden tube ; as this perforated dish floated and remained constantly at the surface of the water, and as the water passing through such a great number (many hundreds) of small holes, was not projected downwards with force, it is evident that by this simple contrivance those violent motions in the mass of water in the jar, which before took place when the hot water was poured in the ice, and when the paper which covered the ice was removed, were, in a great measure, prevented.

In order that the water which was poured through the wooden tube (the bore of which was about half an inch in diameter) might impinge perpendicularly and with force against the bottom of the dish, the lower end of the tube was closed by a fit cork stopple, and the water was made to issue horizontally through a number of small holes in the sides of this tube, at its lower end.

As soon as the operation of pouring the hot water into the jar was finished, the perforated dish was carefully removed, and the jar was covered with a circular wooden cover, from the centre of which a small mercurial thermometer was suspended.

The effects produced by this new arrangement of the machinery will appear by comparing the results of the two following Experiments with those just mentioned.

Number of the Experiment.	Time the hot water remained on the ice.	Temperature of the hot water,		Quantity of ice melted.
		At the beginning.	At the end.	
No. 23	Minutes. 1	196°	196°	Grains. 423
No. 24	3	190°	188°	703

In order still more effectually to prevent the inaccuracies arising from the internal motions in the mass of hot water which were occasioned in pouring the water into the jar, (and which could not fail to affect, more or less, the results of the Experiment,) I had recourse to the following contrivance.

I filled a small phial containing $8\frac{1}{2}$ cubic inches with ice-cold water, and then emptying the phial in the jar, I covered the surface of the ice with this ice-cold water to the height of 0.478 of an inch.

On the surface of this ice-cold water, instead of that of the ice, I now placed the perforated wooden dish previously made ice-cold, and poured the hot water upon it.

The results of the following Experiments show that this contrivance tended much to diminish the apparent irregularities of the Experiments.

The air of the room in which these Experiments were made was of the temperature of 41° .

No. of the Experiment.	Time the hot water was on the ice.	Temperature of the hot water one inch below its surface,		Quantity of ice melted.
		At the beginning	At the end.	
	Minutes.			Grains.
No. 25	10	192°	182°	580
No. 26	30	190°	165°	914
No. 27	180	190°	95°	3200

From the results of these last three Experiments, we can now determine with a very considerable degree of certainty how much ice was melted *in the act of pouring the water into the jar*, and consequently the rate at which it was melted in the ordinary course of the Experiment;—supposing equal quantities to be melted in equal times.

As in the 27th Experiment 3200 grains were melted in 180 minutes, and in the 25th Experiment 580 grains were melted in 10 minutes, we may safely conclude that the same quantity must have been melted in the same time (10 minutes) in the 27th Experiment; if, therefore, from 3200 grains,—the quantity melted in 180 minutes in this last Experiment,—we deduct 580 grains for the quantity melted during the first 10 minutes, there will remain 2620 grains for the quantity melted in the succeeding 170 minutes, when the motions occasioned in the water on its being poured into the jar having subsided, we may suppose the process of melting the ice to have gone on regularly.

But if in the regular course of the Experiment, no more than 2620 grains were melted in 170 minutes,

minutes, it is evident that not more than 154 grains could have been melted in the ordinary course of the process in 10 minutes; for 170 minutes : 2620 grains :: 10 minutes : 154 grains.—If, therefore, from 580 grains, the quantity of ice actually melted in 10 minutes in the 25th Experiment, we deduct 154 grains, there remains 426 for the quantity melted in pouring the water into the jar.

Let us see now how far this agrees with the result of the 26th Experiment. In this Experiment 914 grains of ice were melted in 30 minutes. If from this quantity we deduct 426 grains, the quantity which according to the foregoing computation must have been melted *in pouring the hot water into the jar*, there will remain 478 grains for the quantity melted in the ordinary course of the process in 30 minutes; which gives 159 grains for the quantity melted in 10 minutes; which differs very little from the result of the foregoing computation, by which it appeared to be = 154 grains. This difference however, small as it is, is sufficient to prove an important fact, namely, that the effects produced by the motion into which the hot water had been thrown in being poured into the jar had not ceased entirely in 10 minutes, or when an end was put to the 5th Experiment. We shall therefore come nearer the truth, if, in our endeavours to discover the quantity of ice melted in any given time in the ordinary course of the Experiments, we found our computation on the results of the two Experiments No. 26 and No. 27.

In the latter of these Experiments 3200 grains of ice were melted in 180 minutes, and in the former 914 grains were melted in 30 minutes. If, therefore, from 3200 grains, the quantity melted in 180 minutes, we take the quantity melted in the first 30 minutes, = 914 grains, there will remain 2286 grains for the quantity melted in the succeeding 150 minutes, and this gives 152 grains for the quantity melted in 10 minutes. By the former computation it turned out to be 154 grains.

But if 152 grains of ice is the quantity melted in 10 minutes, in the ordinary course of the process, three times that quantity, or 456 grains only, could have been melted *in this manner* in the 30 minutes during which the 26th Experiment lasted; and deducting this quantity from 914 grains, the quantity actually melted in that Experiment, the remainder, 458 grains, shows how much must have been melted in the pouring the hot water on the ice, or in consequence of the motions into which the water was thrown in the performance of that operation. By the preceding computation this quantity turned out to be 426 grains.

From the result of these computations I think we may safely conclude, that in the ordinary course of the Experiments not more than 152 grains of ice were melted by the hot water in 10 minutes.

I shall now proceed to give an account of several Experiments in which the water employed to melt the ice was at a *much lower temperature*.

Having

Having removed a small quantity of ice which remained unmelted in the bottom of the jar, I put a fresh quantity of water into it, and placing the jar in a freezing mixture, caused this water, which filled the jar to the height of four inches, to freeze into one solid mass of ice. I then placed the jar in a shallow earthen dish, and surrounded it to the height of the level of the top of the ice with a mixture of snow and water (see Plate II.); and placing it in a room in which there had been no fire made for many months, and in which the temperature of the air was at 41° , I let it remain quiet two hours, in order that the ice might acquire the temperature of 32° .

This being done, I took the jar out of the earthen dish, and wiping the outside of it dry with a cold napkin, I weighed the jar with the ice in it very exactly, and then replaced it in the earthen dish, and surrounded it as before with snow and water, to the height of the level of the surface of the ice.

I then poured $73\frac{1}{4}$ ounces Troy ($=15,160$ grains) of water, at the temperature of 41° , into the jar, which covered the ice to the same height to which it had been covered in the former Experiments,—namely, to about 8 inches, and suffering it to stand on the ice a certain number of minutes, I then poured it off, and wiping the outside of the jar, weighed it, in order to ascertain how much ice had been melted.

In putting this cold water into the jar the same precautions were used (by pouring it through the

wooden tube into the perforated wooden dish, &c.) as were used when the Experiment was made with boiling the water.

The following Table shows the results of six Experiments made the same day, (the 19th March 1797,) in the order in which they are numbered, and which were all made with the utmost care:

Number of the Experiment.	Temperature of the water in the jar 1 inch below its surface,		Temperature of the air.	Time the water remained on the ice.	Quantity of ice melted.
	At the beginning of the Exp.	At the end of the Exp.			
No. 28	41°	40°	41°	Minutes. 10	Grains. 203
No. 29	41°	40°	41°	10	220
No. 30	41°	40°	41°	10	237
No. 31	41°	40°	41°	10	228
No. 32	41°	38°	41°	30	617
No. 33	41°	38°	41°	30	585

The agreement in the results of these Experiments is not much less extraordinary than the surprising fact which is proved by them,—namely, that boiling-hot water does not thaw more ice in any given time *when standing quietly on its surface* than water at the temperature of 41°—or nine degrees only above the point of freezing!

There is reason to conclude that it does not even thaw so much;—and this still more remarkable circumstance may, I think, be accounted for in a satisfactory manner, on the supposition (which, however, I imagine will no longer be considered as a bare

a bare supposition) that water is a non-conductor of Heat.

It appeared from the results of the Experiments made with hot water, that the quantity of ice melted in 10 minutes in the ordinary course of that process amounted to no more than 152 grains;—but in these Experiments with cold water, the quantity melted in that time was never less than 203 grains, and taking the mean of four Experiments, it amounted to 222 grains.

There is one circumstance, however, respecting these Experiments with cold water, which it is necessary to investigate before their results can be admitted as complete proof in the important case in question.

In the Experiments which were made with hot water, it was found that a considerable part of the ice which was melted, was melted in consequence of the motions into which the water was thrown upon being poured into the jar, and that the effect of these motions continued to be sensible for a longer time than most of these Experiments with cold water lasted. Is it not possible that the results of these Experiments with cold water may also have been affected by the same cause? This is what I shall endeavour to find out.

In the 32d Experiment 617 grains of ice were melted in 30 minutes, and in the 33d Experiment 585 grains were melted in the same time; and taking the mean of these two Experiments it appears that 601 grains were melted in 30 minutes. If now from this quantity we deduct that which,

according to the mean result of the four preceding Experiments, must have been melted in 10 minutes, namely, 222 grains, there will remain 379 grains for the quantity melted in the last 20 minutes in these two Experiments; consequently, half this quantity, or $189\frac{1}{2}$ grains, is what must have been melted in 10 minutes in the ordinary course of the process.

But this quantity, ($189\frac{1}{2}$ grains,) though less than what was actually melted in the Experiments which lasted only 10 minutes, is still considerably greater than 152 grains, the quantity which was found to have been melted in the same time in the ordinary course of the process in those Experiments in which hot water was used; consequently the great question for the decision of which these Experiments were contrived is,—I believe I may venture to say,—decided.

But, however conclusive the result of these Experiments appeared to me to be, I felt myself too much interested in the subject to rest my inquiries here.

Having found, as well from the results of the Experiments made with cold water as from those made with hot water, that a considerable quantity of ice was melted in the act of pouring the water into the jar, and in consequence of those undulatory motions into which the water was thrown in that operation, notwithstanding all the pains I had taken to diminish those motions and prevent their effects, I now doubled my precautions in guarding against those sources of error and uncertainty.

Before

Before I poured the water into the jar I covered the surface of the ice to the height 0.956 of an inch with ice-cold water, and this I did when water at the temperature of 41° was used, as well as in those Experiments in which boiling-hot water was employed. In the former Experiments I had covered the surface of the ice with ice-cold water only in those Experiments in which hot water was used, and even in those I used only half as much ice-cold water as I now employed for that purpose.

I also now poured the water into the jar in a smaller stream, employing no less than three minutes in filling it up to the height of eight inches above the surface of the ice; and I endeavoured to ascertain how far the results of the Experiments were influenced by the temperature of the air, and also by wrapping up the jar in warm covering.

The same jar was used in all the Experiments, and it was always placed in the same earthen dish, and surrounded, to the level of the top of the ice, with melting snow. This jar is very regular in its form, being very nearly a perfect cylinder, and is on that account peculiarly well calculated for the use for which I selected it.

In each of the three first Experiments, which are entered in the following Table, the jar was well covered up with a very warm covering of cotton wool. This covering (which was above an inch thick) reached from the surface of the melting snow in which the jar stood, quite to the top of the jar. The mouth of the jar was first covered with a round wooden cover, (from the centre of which a

thermometer, the bulb of which reached one inch below the surface of the water, was suspended,) and on the top of this wooden cover there was put a thick covering of cotton.

In all the Experiments in the following Table, except the three first, the jar was exposed naked to the air, except the lower part of it, which, as I have already more than once observed, was always covered, as high as the ice in the jar reached, with melting snow, or with pounded ice and water.

In the two Experiments No. 37 and No. 38, which are marked with asterisks, the surface of the ice was covered with ice-cold water to the depth of 0.478 of an inch only;—in all the other Experiments it was covered to the depth of 0.956 of an inch.

Number of the Experi- ment.	Temperature of the water in the jar 1 inch below its surface,		Tempera- ture of the air.	Time the water re- mained on the ice.	Quantity of ice melted.
	In the be- ginning of the Exp.	At the end of the Exp.			
				Minutes.	Grains.
No. 34	188°	179°	41°	30	634
No. 35	189°	180°	41°	30	747
No. 36	190°	147°	41°	180	3963
No. 37	41°	38°	41°	30	592 *
No. 38	41°	43°	61°	30	676 *
No. 39	186°	157°	61°	30	559
No. 40	188°	156°	61°	30	575
No. 41	190°	156°	61°	30	542
No. 42	41°	43°	61°	30	573
No. 43	42°	44°	61°	30	575
No. 44	42°	35°	61°	120	2151

The results of these Experiments afford matter for much curious speculation, but I shall content myself for the present with making only two or three observations respecting them. And in the first place, it is remarkable, that although in the Experiments No. 34 and No. 35, of 30 minutes each, considerably less ice was melted than in that No. 26, which lasted the same time; yet, in that No. 36, of 180 minutes, more was melted than in that No. 27, of the same duration. This difference in the two last-mentioned Experiments will be accounted for hereafter.

With regard to the difference in the results of the Experiments of 30 minutes, there is no doubt but that it arose from the precautions which had been

been taken in this last set of Experiments to prevent the effect of the violent motions into which the hot water was thrown in being poured into the jar, that less ice was melted in the Experiments No. 34 and No. 35, than in that No. 26.

Secondly, It appears that more ice was melted in the same time, in the Experiments in which the jar was covered up with warm covering, than in those in which it was left naked and exposed to the air of the room.

The difference is even considerable. The quantity melted in 30 minutes when the jar was covered, at a mean of two Experiments, (No. 34 and No. 35,) was $690\frac{1}{2}$ grains;—but when the jar was naked, the quantity at a mean of three Experiments (No. 39, No. 40, and No. 41) was only $558\frac{2}{3}$ grains.

Thirdly, The quantity of ice melted under similar circumstances,—that is to say, when the jar was naked,—was sensibly greater when the water was at the temperature of about 41° , than when it was nearly boiling hot. In the Experiment No. 41, when the water which was poured on the ice was at the temperature of 190° deg. 542 grains only of ice were melted in 30 minutes; whereas, in the next Experiment, (No. 42,) when the water was at 41° , or 149 degrees colder, 573 grains were melted in the same time.

Finding that covering up the jar with a thick and warm covering of cotton caused more ice to be melted by the hot water, I was curious to see what effects would be produced by keeping the jar plunged

plunged *quite up to its brim* in a mixture of snow and water, instead of merely surrounding that part of it which was occupied by the cake of ice by this cold mixture.

I was likewise desirous of finding out,—and if possible at the same time,—whether water, at a temperature something above that at which that Fluid ceases to be condensed with cold, would not melt more ice in any given time than an equal quantity of that Fluid, either colder or much hotter. The result of the 43d Experiment had shewn me,—what indeed a very simple computation would have pointed out,—namely, that when the temperature of the water is but a few degrees above the point of freezing, if its quantity or depth is not very considerable, it will soon be so much cooled as very sensibly to retard the process of melting the ice: and with respect to hot water, the increased quantity of ice which was melted by it when the jar was covered up with a warm covering, convinced me that the real cause which prevented the hot water from melting as much ice as the cold water in my Experiments, was the embarrassments in the process of melting the ice, which were occasioned by the descending currents formed in the hot water on its being cooled by the air at its surface, and by the sides of the jar.

These descending currents meeting in the region of the constant temperature of 40° with those cold currents which ascended from the surface of the ice, it seems very probable that the ascending
currents,

currents, on the motion of which the melting of ice depends, were checked by this collision.

By retarding the cooling of the hot water above, by wrapping up the jar in a warm covering, the velocity of the descending currents was of course diminished; but when this was done, the results of the Experiment showed that the melting of the ice was accelerated.

When the jar being naked, the cooling of the hot water, and consequently the motions of the descending currents, were rapid, no more than about 542 grains, or at most 575 grains, were melted in 30 minutes; but when the jar was covered with a warm covering, 634 grains, and in one Experiment (that No. 35) 747 grains, were melted in the same time.

As plunging the jar in a cold mixture of snow and water could not fail to accelerate the cooling of the hot water in the jar, and consequently to increase the rapidity of the descending currents in it, ought not this to embarrass, in an extraordinary degree, the ascending currents of ice-cold water from the surface of the ice, and to diminish the quantity of ice melted?—This is what the following Experiments, compared with the results of those No. 39, No. 40, and No. 41, will show.

Number of the Experiment.	Temperature of the water in the jar 1 inch below its surface,		Temperature of the cold mixture in which the jar was kept plunged to its brim.	Time the water remained on the ice.	Quantity of ice melted.
	In the beginning of the Exp.	At the end of the Exp.			
No. 45	188°	68°	32°	30	406
No. 46	186°	67°	32°	30	440
No. 47	189°	68°	32°	30	432
No. 48	187°	67°	32°	30	355
No. 49	188°	68°	32°	30	364

Quantity of ice melted in these 5 Experiments, 1997

Mean quantity melted by hot water when the jar was kept plunged to its brim in melting ice and water, - 399½ Grains.

Mean quantity melted by hot water in 30 minutes, in the two Experiments, No. 26 and No. 27, when the part of the jar occupied by the water was surrounded by air, at the temperature of 41°, - 456

Mean quantity melted by hot water in 30 minutes, in the three Experiments, No. 39, No. 40, and No. 41, when the part of the jar occupied by the water was surrounded by air, at the temperature of 61°, - 558½

Mean quantity melted by hot water in 30 minutes, in the two Experiments, No. 34 and No. 35, when the part of the jar occupied by the water was kept covered up by a thick and warm covering of cotton, - 690½

As all the Experiments were made in the same manner, and with equal care, and differed only in respect to the manner in which the outside of the jar, above the surface of the ice in it, was covered, their results show the effects produced by those differences.

I should perhaps have suspected that the greater quantity of ice which was melted when the heat of the water in the jar was confined for the longest time had been occasioned, at least in part, by the Heat communicated downwards by the medium of the glass; but that this could not have been the case was evident, not only from the manner in which the ice was always found to have been melted, but also from the results of similar Experiments made with much colder water.

Had it been melted by Heat communicated by the glass, it would undoubtedly have been most melted in those parts of its surface where it was in contact with the glass, but this I never once found to be the case.

The results of the following Experiments will show,—what indeed might easily have been foreseen,—that the temperature of the medium by which the upper part of the jar was surrounded does not always affect the result of the Experiment in the same degree,—nor even always in *the same manner*,—in different Experiments in which the temperature of the water in the jar is very different.

To facilitate the comparison of these Experiments, and that of the foregoing, which are similar to them, I shall here place them together.

Number of the Experiment.	Temperature of the water in the jar 1 inch below its surface,		Temperature of the medium by which the upper part of the jar was surrounded.	Time the water remained on the ice.	Quantity of ice melted.
	In the beginning of the Exp.	At the end of the Exp.			
No. 50.	41°	36°	32°	30	542
No. 37	41°	38°	41°	30	592
No. 42	41°	43°	61°	30	576

It is certainly very remarkable indeed that so much more ice should be melted by water at the temperature of 41°, when the jar which contained it was surrounded by a cold mixture of pounded ice and water, than by an equal quantity of boiling-hot water in the same circumstances. In the Experiment No. 50, the quantity melted by the cold water was 542 grains, while that melted by the boiling-hot water, taking the mean of five Experiments, (those No. 45, 46, 47, 48, and 49,) was no more than $399\frac{2}{3}$ grains. But the results of the four following Experiments are, if possible, still more surprising.

These Experiments were made with water at the temperature of 61°, the temperature of the air of the room being at the same time 61°; in the two first of these Experiments the jar was kept plunged to its brim in a mixture of snow and water,—in the two last its lower part only, namely, as high as the level of the surface of the ice, was surrounded

by this cold mixture, its upper part being naked, and surrounded by the air of the room.

In each of the Experiments, (as in those which preceded them,) before the water was poured into the jar the surface of the ice was covered to the height of 0.956 of an inch with ice-cold water, in order more effectually to defend it against the effects of the temporary motions into which the water employed to melt the ice was unavoidably thrown in the performance of this operation; and the same quantity of water was always used, namely, $73\frac{1}{4}$ ounces Troy, or as much as was sufficient to fill the jar to the height of 8 inches.

Number of the Experiment.	Temperature of the water in the jar 1 inch below its surface,		Temperature of the medium by which the upper part of the jar was surrounded.	Time the water remained on the ice.	Quantity of ice melted.
	In the beginning of the Exp.	At the end of the Exp.			
No. 51	61°	49°	32°	30	660
No. 52	61°	50°	32°	30	662
No. 53	61°	60°	61°	30	642
No. 54	61°	60°	61°	30	650

These Experiments are remarkable, not only on account of the very small difference in the quantities of ice melted which resulted from the cooling of the sides of the jar, but also, and more especially, as that difference was directly contrary to the effects produced by the same means in the Experiments with hot water. More ice was melted when the

the outside of the jar was kept ice-cold than when it was surrounded by air at the temperature of 61° .

All these appearances might, I think, be accounted for in a satisfactory manner on the principles we have assumed respecting the manner in which Heat is propagated in liquids; but without engaging ourselves at present too far in these abstruse speculations, let us take a retrospective view of all our Experiments, and see what general results may with certainty be drawn from them.

One of the Experiments in which the greatest quantity of ice was melted by *hot water* is that No. 36, in which 3963 grains were melted in three hours, or 180 minutes. If now from this quantity we deduct that which, according to the results of the two preceding Experiments, must have been melted in the first 30 minutes, namely, $690\frac{1}{2}$ grains, there will remain $3272\frac{1}{2}$ grains for the quantity melted in the last 150 minutes, which gives $654\frac{1}{2}$ grains for the quantity melted in 30 minutes *in the ordinary course of the Experiment*.

This quantity, $654\frac{1}{2}$ grains, deducted from that which at a mean of two Experiments (those No. 34 and No. 35) was found to be actually melted in 30 minutes, namely, $690\frac{1}{2}$ grains, leaves 36 grains for the quantity which in these two Experiments was melted in consequence of the temporary motions into which the hot water was thrown in the operation of pouring it into the jar. The difference between these two quantities ($= 36$ grains) is very

inconsiderable, and shows that the means used for diminishing the effects produced by those motions had been very efficacious.

As the results of the three Experiments No. 34, No. 35, and No. 36, were exceedingly regular and satisfactory,—as the Heat of the water appears to have been so completely confined by the warm covering which surrounded the jar, and as the process of melting the ice went on regularly or equally for so great a length of time (three hours) in the 36th Experiment, we may venture to conclude that more ice could not possibly have been melted by boiling-hot water—*standing on it*—than was melted in these Experiments.

This quantity was found to be at the rate of $654\frac{1}{2}$ grains in 30 minutes.

But as in these three Experiments extraordinary means were used, by which an uncommonly large quantity of ice was melted, they cannot be considered as similar to those which were made with cold water, and consequently cannot with propriety be compared with them.

When the Experiments were similar, the mean results of those which were made with water at different temperatures were as follows :

		Ice melted in 30 minutes
		Grains.
In the Experiments in which the part of the jar which was occupied by the water was exposed uncovered to the air at the temperature of 61°	With boiling-hot water (Experiments No. 39, 40, and 41)	558 $\frac{1}{2}$
	With water at the temperature of 61° (Experiments No. 53 and No. 54)	646
	With water at the temperature of 41° (Experiments No. 42 and No. 43)	574
In the Experiments in which the part of the jar which was occupied by the water was surrounded by pounded ice and water, and consequently was at the temperature of 32°	With boiling-hot water, (Experiments No. 45, 46, 47, 48, and 49)	399 $\frac{1}{2}$
	With water at the temperature of 61° (Experiments No. 51 and No. 52)	661
	With water at the temperature of 41° (Experiment No. 50)	542

From the results of all these Experiments we may certainly venture to conclude that boiling-hot water is not capable of melting more ice *when standing on its surface*, than an equal quantity of water at the temperature of 41°, or when it is only *nine degrees* above the temperature of freezing!

This fact will, I flatter myself, be considered as affording the most unquestionable proof that could well be imagined, that water is a perfect *non-conductor of Heat*, and that Heat is propagated in it *only* in consequence of the motions which the

Heat occasions in the insulated and solitary particles of that fluid *.

The discovery of this fact opens to our view one of the most interesting scenes in the economy of Nature:—but in order to prepare our minds for the contemplation of it, it will be not amiss to refresh our memory by recapitulating what has already been said on the Propagation of Heat in Fluids, and particularly in water; and adding such occasional observations as may tend to elucidate that abstruse subject.

Those who enter into the spirit of these investigations will not consider these repetitions and illustrations as either superfluous or tiresome.

* The insight which this discovery gives us in regard to the nature of the mechanical process which takes place in chemical solutions is too evident to require illustration;—and it appears to me that it will enable us to account in a satisfactory manner for all the various phenomena of chemical affinities and vegetation. Perhaps all the motions among inanimate bodies on the surface of the globe may be traced to the same cause,—namely, to the non-conducting power of Fluids with regard to Heat.

C H A P. III.

Recapitulation, and farther Investigation of the Subject.—All Bodies are condensed by Cold without Limitation, WATER ONLY EXCEPTED.—Wonderful Effects produced in the World in consequence of the particular Law which obtains in the Condensation of Water.—This Exception to one of the most general Laws of Nature, a striking Proof of Contrivance in the Arrangement of the Universe;—a Proof which comes home to the Feelings of every ingenuous and grateful Mind.—This particular Law does not obtain in the Condensation of SALT-WATER.—Final Cause of the Saltiness of the Sea—The Ocean probably designed by the Creator to serve as an Equalizer of Heat—Could not have answered that Purpose had its Waters been fresh.—Final Cause of the Freshness of Lakes and inland Seas in high Latitudes.—Usefulness of these Speculations.

As the immediate cause of the motions in a liquid, which take place on its undergoing a change of temperature, is evidently the change in the specific gravity of those particles of the liquid which become either hotter or colder than the rest of the mass; and as the specific gravities of some liquids are much more changed by any given change of temperature than that of others, ought

not this circumstance (independent of the more or less perfect fluidity of the liquid) to make a sensible difference in the conducting power of liquids?

The more a liquid is expanded by any given change of temperature, the more rapid will be the ascent of the particles which first receive the Heat; and as these are immediately replaced by other colder particles, which, in their turns, come to be heated, this must of course produce a rapid communication of Heat from the hot body of the liquid.

But when, on the other hand, the specific gravity of a liquid is but little changed by any given change of temperature, the motions among the particles of the liquid occasioned by this change must be very sluggish, and the communication of Heat of course very slow.

Let us stop here for one moment just to ask ourselves a very interesting question. Suppose that in the general arrangement of things it had been necessary to contrive matters so that water should not freeze in winter,—or that it should not freeze *but with the greatest difficulty*;—very slowly;—*and in the smallest quantity possible*;—How could this have been most readily effected?—

Those who are acquainted with the law of the condensation of Water on parting with its Heat have already anticipated me in these speculations; and it does not appear to me that there is any thing which human sagacity can fathom, within the wide-extended bounds of the visible creation, which affords a more striking or more palpable
proof

proof of the wisdom of the Creator, and of the special care he has taken in the general arrangement of the universe to preserve animal life, than this wonderful contrivance:—for though the extensiveness and immutability of the general laws of Nature impress our minds with awe and reverence for the Creator of the universe, yet *exceptions to those laws*, or particular modifications of them, from which we are able to trace effects evidently *salutary* or advantageous to ourselves and our fellow-creatures, afford still more striking proofs of contrivance, and ought certainly to awaken in us the most lively sentiments of admiration, love, and gratitude.

Though in temperatures above blood-heat the expansion of water with Heat is very considerable, yet in the neighbourhood of the freezing point it is almost nothing. And what is still more remarkable, as it is an exception to one of the most general laws of Nature with which we are acquainted, when in cooling it comes within eight or nine degrees of Fahrenheit's scale of the freezing point, instead of going on to be farther condensed as it loses more of its Heat, it *actually expands* as it grows colder, and continues to expand more and more as it is more cooled.

If the whole amount of the condensation of any given quantity of boiling-hot water, on being cooled to the point of freezing, be divided into any given number of equal parts, the condensations corresponding to equal changes of temperature will be very unequal in different temperatures.

In cooling $22\frac{1}{2}$ degrees of Fahrenheit's scale, (or one-eighth part of the interval between the boiling and the freezing points,) the condensation will be,

			Condensation.
In cooling $22\frac{1}{2}$ ° viz. from 212° to $189\frac{1}{2}^{\circ}$	-	18	parts.
$189\frac{1}{2}^{\circ}$ — 167°	-	16.2	
167° — $144\frac{1}{2}^{\circ}$	-	13.8	
$144\frac{1}{2}^{\circ}$ — 122°	-	11.5	
122° — $99\frac{1}{2}^{\circ}$	-	9.3	
$99\frac{1}{2}^{\circ}$ — 77°	-	7.1	
77° — $54\frac{1}{2}^{\circ}$	-	3.9	
$54\frac{1}{2}^{\circ}$ — 32°	-	0.2	

Hence it appears that the condensation of water, or increase of its specific gravity in being cooled $22\frac{1}{2}$ degrees of Fahrenheit's scale, is at least *ninety times greater* when the water is boiling-hot, than when it is at the mean temperature of the atmosphere in England ($54\frac{1}{2}^{\circ}$), or within $22\frac{1}{2}$ degrees of freezing—(for 18 is to 0.2 as 90 to 1).

All liquids, it is true, in cooling, are more condensed by any given change of temperature when they are very hot, than when they are colder; but these differences are nothing compared to those we observe in water.

The ratio of the condensation in cooling from 212° to $189\frac{1}{2}^{\circ}$ to that in cooling from $54\frac{1}{2}^{\circ}$ to 32° in each of the under-mentioned fluids, has been shown, by the Experiments of M. DE LUC, to be as follows:

Olive oil	-	-	as $1\frac{14}{100}$ to 1
Strong spirits of wine	-	-	as $1\frac{29}{100}$ to 1
A saturated solution of sea-salt in water	-	}	as $1\frac{38}{100}$ to 1

The

The difference between the laws of the condensation of pure water, and of the same fluid when it holds in solution a portion of salt, is striking; but when we trace *the effects* which are produced in the world by that arrangement, we shall be lost in wonder and admiration,

Let me beg the attention of my reader while I endeavour to investigate this most interesting subject, and let me at the same time bespeak his candour and indulgence. I feel the danger to which a mortal exposes himself who has the temerity to undertake to explain the designs of Infinite Wisdom.—The enterprise is adventurous, but it cannot surely be improper.

The wonderful simplicity of the means employed by the Creator of the world to produce the changes of the seasons, with all the innumerable advantages to the inhabitants of the earth which flow from them, cannot fail to make a very deep and a lasting impression on every human being whose mind is not degraded and quite callous to every ingenuous and noble sentiment: but the farther we pursue our inquiries respecting the constitution of the universe, and the more attentively we examine the effects produced by the various modifications of the active powers which we perceive, the more we shall be disposed to admire, adore, and love that great First Cause which brought all things into existence.

Though winter and summer, spring and autumn, and all the variety of the seasons, are produced in a manner at the same time the most

simple and the most stupendous (by the inclination of the axis of the earth to the plane of the ecliptic); yet this mechanical contrivance alone would not have been sufficient (as I shall endeavour to show) to produce that gradual change of temperature in the various climates which we find to exist, and which doubtless is indispensably necessary to the preservation of animal and vegetable life.

Though change of temperature seems necessary to the growth and perfection of most vegetables, yet these changes must be within certain limits. Some plants can support greater changes of temperature than others, but the extremes of Heat and of Cold are alike fatal to all.

As the rays of the sun are the immediate cause of the Heat on the surface of the globe, and as the length of the days in high latitudes is so very different in summer and in winter, it is evident that, in order to render those regions habitable, some contrivance was necessary to prevent the consequences which this great inequality of the Heat generated by the sun in summer and in winter would naturally tend to produce; or, in other words, to equalize the Heat, and moderate its extremes in these two seasons.

Let us see how far *Water* is concerned in this operation, and then let us examine how far the remarkable law which has been found to obtain in its condensation with cold tends to render it well adapted to answer that most important purpose.

The

The vast extent of the ocean, and its great depth,—but still more its numerous currents, and the power of water to absorb a vast quantity of Heat, render it peculiarly well adapted to serve as an equalizer of Heat.

On the retreat of the sun after the solstice, it is closely followed by the cold winds from the regions of eternal frost, which are continually endeavouring to press in towards the equator. As the power of the sun to warm the surface of the earth and the air diminishes very fast in high latitudes on the days growing shorter, it soon becomes too weak to keep back the dense atmosphere which presses on from the polar regions, and the cold increases very fast.

There is, however, a circumstance by which these rapid advances of winter are in some measure moderated. The earth, but more especially the *water*, having imbibed a vast quantity of Heat during the long summer days, while they received the influence of the sun's vivifying beams; this Heat, being given off to the cold air which rushes in from the polar region, serves to warm it and soften it, and consequently to diminish the impetuosity of its motion, and take off the keenness of its blast. But as the cold air still continues to flow in as the sun retires, the accumulated Heat of summer is soon exhausted; and all solid and fluid bodies are reduced to the temperature of freezing water. In this stage the cold in the atmosphere increases very fast, and would probably increase still faster, were it not for the vast quantity of Heat which is communicated

municated to the air by the watery vapours which are first condensed, and then congealed, in the atmosphere, and which afterwards fall upon the earth in the form of snow; and by that still larger quantity which is given off by the water in the rivers and lakes, and in the ground upon its being frozen.

But in very cold countries, the ground is frozen and covered with snow, and all the lakes and rivers are frozen over in the very beginning of winter. The cold then first begins to be extreme, and there appears to be no source of Heat left, which is sufficient to moderate it in any sensible degree.

Let us see what must have happened if things had been left to what might be called their natural course;—if the condensation of water on being deprived of its Heat had followed the law which we find obtains in other fluids, and even in water itself in some cases, namely, when it is mixed with certain bodies.

Had not Providence interfered on this occasion in a manner which may well be considered as *miraculous*,—all the fresh water within the polar circle must inevitably have been frozen to a very great depth in one winter, and every plant and tree destroyed; and it is more than probable that the regions of eternal frost would have spread on every side from the poles, and, advancing towards the equator, would have extended its dreary and solitary reign over a great part of what are now the most fertile and most inhabited climates of the world!

In latitudes where now the return of spring is hailed by the voice of gladness,—where the earth
decks

decks herself in her gayest attire; and millions of living beings pour forth their songs of joy and gladness, nothing would have been heard but the whistling of the rude winds,—and nothing seen but ice and snow, and flying clouds charged with wintry tempests.

Let us, with becoming diffidence and awe, endeavour to see what the means are which have been employed by an almighty and benevolent God to protect his fair creation.

As nourishment and life are conveyed to all living creatures through the medium of water;—*liquid*,—*living* water;—to preserve life, it was absolutely necessary to preserve a great quantity of water in a fluid state, in winter as well as in summer.

But in cold climates the temperature of the atmosphere, during many months in the year, is so much below the freezing point, that, had not measures been taken to prevent so fatal an accident, all the water must inevitably have been changed to ice, which would infallibly have caused the destruction of every living thing.

Extraordinary measures were therefore necessary for preserving in a liquid state as much of the water existing in those climates as is indispensably necessary for the preservation of vegetable and animal life; and this could only be done by contriving matters so as to prevent this water from parting with its Heat to the cold atmosphere.

It has been shown,—I believe I may venture to say proved,—in the most satisfactory manner,—
that

that liquids part with their Heat **ONLY** in consequence of their internal motions;—and that the more rapid these motions are, the more rapid is the communication of the Heat;—that these motions are produced by the change in the specific gravity of the liquid, occasioned by the change of temperature,—and of course that they are more rapid, as the specific gravity of the liquid is the more changed by any given change of temperature.

But it has been shown that the change in the specific gravity of water is extremely small, which takes place in any given change of temperature, *below the mean temperature of the atmosphere*; and particularly when the temperature of the water is very near the freezing point; and hence it follows, that water must give off its Heat very slowly when it is near freezing.

But this is not all. There is a still more extraordinary, and in its consequences more wonderful, circumstance which remains to be noticed. When water is cooled to within eight or nine degrees of the freezing point, it not only ceases to be farther condensed, but is actually expanded by farther diminutions of its Heat; and this expansion goes on as the Heat is diminished, as long as the water can be kept fluid; and when it is changed to ice, it expands even still more, and the ice floats on the surface of the uncongealed part of the Fluid.

Let us see how very powerfully this wonderful contrivance tends to retard the cooling of water when it is exposed in a cold atmosphere.

It is well known that there is no communication of Heat between two bodies as long as they are both at the same temperature; and it is likewise known that the *tendency* of Heat to pass from a hot body into one which is colder, with which it is in contact, is greater, as the difference is greater in the temperatures of the two bodies.

Suppose now that a mass of very cold air reposes on the quiet surface of a large lake of fresh water, at the temperature of 55° of Fahrenheit's thermometer. The particles of water at the surface on giving off a part of their Heat to the cold air with which they are in contact, and in consequence of this loss of Heat becoming specifically heavier than those hotter particles on which they repose, must of course descend. This descent of the particles which have been cooled necessarily forces other hotter particles to the surface, and these being cooled in their turns bend their course downwards; and the whole mass of water is put into motion, and continues in motion as long as the process of cooling goes on.

Before I proceed to trace this operation through all its various stages, I must endeavour to remove an objection which may perhaps be made to my explanation of this phænomenon. As I have supposed the mass of air which rests on the surface of the water to be *very cold*, and as I have taken it for granted that there is no communication whatever of Heat between the particles of water in contact with this very cold air, and the neighbouring warmer particles of water, it may be asked,
how

how it happens that these particles at the surface are not so much cooled as to be immediately changed to ice? To this I answer, that there are two causes which conspire to prevent the *immediate* formation of ice at the surface of the water: *First*, the specific gravity of the particle of water at the surface being increased at the same moment when it parts with Heat, it begins to descend as soon as it begins to be cooled; and before the air has had time to rob it of all its Heat, it escapes and gets out of its reach: And *secondly*, air being a bad conductor of Heat, it cannot receive and transmit or *transport* it with sufficient celerity to cool the surface of water so suddenly as to embarrass the motions of the particles of that liquid in the operation of giving it off.

But to return to our lake: As soon as the water in cooling has arrived at the temperature of about 40° , as at that temperature it ceases to be farther condensed, its internal motion ceases, and those of its particles which happen to be at its surface remain there; and after being cooled down to the freezing point, they give off their latent Heat, and ice begins to be formed.

As soon as the surface of the water is covered with ice, the communication of Heat from the water to the atmosphere is rendered extremely slow and difficult; for ice being a *bad conductor of Heat* forms a very warm covering to the water,—and moreover it prevents the water from being agitated by the wind. Farther, as the temperature of the ice at its lower surface is always very nearly the same as that

that of the particles of liquid water with which it is in contact, (the warmer particles of this Fluid, in consequence of their greater specific gravity, taking their places below,) the communication of Heat between the water and the ice is necessarily very slow on that account.

As soon as the upper surface of the ice is covered with snow, (which commonly happens soon after the ice is formed,) this is an additional and very powerful obstacle to prevent the escape of the Heat out of the water; and though the most intense cold may reign in the atmosphere, the increase of the thickness of the ice will be very slow.

During this time, the mass of water which remains unfrozen will lose *no part of its Heat*; on the contrary, it will continually be receiving Heat from the ground. This Heat, which is accumulated in the earth during the summer, will not only serve, in some measure, to replace that which is communicated to the atmosphere through the ice, and prevent its being furnished at the expence of the latent Heat of the water in contact with its surface, but when the temperature of the air is not much below that of freezing, this supply of Heat from below will be quite sufficient to replace that which the air carries off; and the thickness of the ice will not increase.

Whenever the temperature of the air is not actually *colder* than freezing water, the Heat which rises from the bottom of the lake will be all employed in melting the ice at its under surface, and diminishing its thickness.

It will indeed frequently happen, when the ice is very thick, and especially when its upper surface is covered with deep snow, that the melting of the ice at its under surface will be going on, when the temperature of the atmosphere is considerably below the freezing point.

As the particles of water which, receiving Heat from the ground at the bottom of the lake, acquire a higher temperature than that of 40° , and being *expended*, and becoming specifically lighter by this additional Heat, rise up to the upper surface of the fluid water, and give off their sensible Heat to the under surface of the ice, never return to the bottom, this communication of the Heat which exhales from the earth produces very little motion in the mass of the water; and this circumstance is, no doubt, very favourable to the preservation of the Heat of the water.

When a strong wind prevails, and the surface of the water is much agitated, ice is not formed, even though the whole mass of water should, by a long continuance of cold weather, have been previously cooled down to that point to which it is necessary that it should be brought, in order that its internal motions may cease, and it may be disposed to congeal; for though the particles at and near the surface may no longer have any tendency to descend, on being farther cooled, yet, as they have so considerable a quantity of sensible Heat (eight or ten degrees) to dispose of, after their condensation with cold ceases, and as the agitation into which the water is thrown by the wind does
not

not permit any particle to remain long enough in contact with the cold air to give off all its Heat at once, there is a continual succession of fresh particles at the surface, all of which give off Heat to the air; but none of them have time to be cooled sufficiently to be disposed to form ice. The water will lose a vast quantity of Heat, and as soon as the wind ceases, if the cold should continue, ice will be formed very rapidly.

But it is not merely the agitation of the water which renders the communication of the Heat very rapid, the agitation of the wind also tends to produce the same effect.

On the return of spring, the snow melting before the sun as he advances and his rays become more powerful, all the Heat which the earth exhales is employed in dissolving the ice at its under surface, while the sun on the other side acts still more powerfully to produce the same effect.

Though ice is transparent, yet it is not perfectly so, and as the light which is stopped in its passage through it cannot fail to generate Heat *when* and *where* it is stopped, or absorbed, it is by no means surprising that snow should be found to melt when exposed in the sun's rays, even when the temperature of the air in the shade is considerably below the point of freezing. Snow exposed to the sun melts long before the even surface of ice begins to be sensibly softened by its beams, and it is not till some time after all the hills are bare that the ice on the lakes and rivers breaks up.

The rays which penetrate a bank of snow being often reflected and refracted, descend deep into it, and the Heat is deposited in a place where it is not exposed to be carried off by the cold air of the atmosphere; but the rays which fall upon the horizontal and smooth surface of ice, are mostly reflected upwards into the atmosphere; and if any part of them are stopped at the surface of the ice, the Heat generated by them *there* is instantaneously carried off by the cold air, and a particle of water is no sooner made fluid than it is again frozen.

Hence we see that the snow which in cold countries covers the ice that is formed on the surface of fresh water, not only prevents the Heat of the water from being carried off by the air, during the winter, but also assists very powerfully in thawing the ice early in the spring.

Let us now see what the consequences would have been had the condensation of water with cold followed the law which obtains in regard to all other Fluids.

As the internal motion of the water could not have failed to continue as long as its specific gravity continued to be increased by parting with Heat, ice would not have begun to be formed till the whole mass of water had arrived at the temperature of 32° of Fahrenheit's thermometer.

To see what an enormous quantity of Heat would be lost when the water is deep in consequence of its whole mass being cooled in this manner, we have only to compute how much ice this Heat

would melt, or how much water it would heat from the point of freezing to that of boiling.

It has been shown by experiment, that any given quantity of ice requires as much Heat to melt it as an equal quantity of fluid water loses in cooling 140 degrees, consequently the quantity of ice which might be melted by the Heat given off by any given quantity of water in cooling any given number of degrees, is to the given quantity of water, as the number of degrees which it is cooled, to 140 degrees.

Hence it follows that when the temperature of the water is 8 degrees above the freezing point, it gives off in cooling down to that temperature as much Heat as would melt $\frac{8}{140}$ or $\frac{2}{35}$ of its weight of ice; the water, therefore, which is cooled from the temperature of 40° to that of 32°, if it be 35 feet deep, will give off as much Heat in being so cooled as would melt a covering of ice 2 feet thick.

But this even is not all, for as the particles of water on being cooled at the surface would in consequence of the increase of their specific gravity, on parting with a portion of their Heat, immediately descend to the bottom, the greatest part of the Heat accumulated during the summer in the earth on which the water reposes would be carried off and lost, before the water began to freeze; and when ice was once formed, its thickness would increase with great rapidity, and would continue increasing during the whole winter; and it seems very probable that in climates which are now tem-

perate, the water in the large lakes would be frozen to such a depth in the course of a severe winter that the Heat of the ensuing summer would not be sufficient to thaw them; and should this once happen, the following winter could hardly fail to change the whole mass of its waters to one solid body of ice, which never more could recover its liquid form, but must remain immovable till the end of time.

In the month of February, after a frost which had lasted a month, the temperature of the air being 38° , M. DE SAUSSURE found the temperature of the water of the Lake of Geneva, at the surface, at 41° ,—and at the depth of 1000 feet, at 40° . Had the frost continued but a little longer, ice would have been formed; but had the constitution of water been such that the whole mass of that Fluid in the Lake must have been cooled down to the temperature of 32° before ice could have been formed, this event could not have happened till the water had given off as much Heat as would be sufficient to melt a covering of ice above 57 feet thick!—

This quantity of Heat would be sufficient to heat, to the point of boiling, a quantity of ice-cold water as large as the Lake, and 49 feet deep.

We cannot sufficiently admire the simplicity of the contrivance by which all this Heat is saved. It well deserves to be compared with that by which the seasons are produced; and I must think that every candid inquirer, who will begin by divesting himself of all unreasonable prejudices, will agree

with me in attributing them both TO THE SAME AUTHOR.

When we trace still farther the astonishing effects which are produced in the world by the operations of that simple law which has been found to obtain in the condensation of water on its being deprived of Heat, we shall find more and more reason to admire the wisdom of the contrivance.

That high latitudes might be habitable, it was necessary that vegetables should be protected from the effects of the chilling frosts of a long and severe winter : but if it be true that watery liquids do not part with their Heat but in consequence of their internal motions ; and if these motions are occasioned merely by the change produced in the specific gravity of those particles of the liquid which receive Heat, or which part with it, who does not see how very powerfully the sudden diminution and final cessation of the condensation of water in cooling, as soon as its temperature approaches to the freezing point, operates to prevent the sap in vegetables from being frozen ?

But if, for the purposes of life and vegetation, it be necessary that the ground, the rivers, the lakes, and the trees be defended from the cold winds from the poles, it may be asked how this innundation of cold air is to be warmed ?—I answer by the waters of the ocean, which there is the greatest reason to think were not only designed principally for that use, but particularly *prepared* for it.

Sea water contains a large proportion of salt in solution, and we have seen that the condensation of a saline solution, on its being cooled, follows a law which is extremely different from that observed in regard to pure water; and which (as may easily be shown) renders it peculiarly well adapted for communicating Heat to the cold winds which blow over its surface.

As sea water continues to be condensed as it goes on to cool, even after it has passed the point at which fresh water freezes, the particles at the surface, instead of remaining there, after the mass of the water had been cooled to about 40° , and preventing the other warmer particles below from coming in their turns and giving off their Heat to the cold air, (as we have seen always happens when fresh or pure water is so cooled,) these cooled particles of *salt water* descend as soon as they have parted with their Heat, and in moving downward force other warmer particles to move upwards; and in consequence of this continual succession of warm particles which come to the surface of the sea, a vast deal of Heat is communicated to the air;—incomparably more than could possibly be communicated to it by an equal quantity of fresh water at the same temperature, as will appear by the following computation.

Without taking into the account that very great advantage which sea water possesses over fresh water, considered as an equalizer of the temperature of the atmosphere, which arises from the comparative *lowness of the point of its congelation*;—supposing

posing even sea water to freeze at as high a temperature as fresh water, namely, at 32° ; and supposing (what is strictly true) that as soon as either sea water or fresh water is frozen at its surface, and this ice covered with snow, that the communication of Heat from the water to the atmosphere ceases almost entirely;—we will endeavour to determine how much more Heat would, even on this supposition, be communicated to the air by salt water than by fresh water, after both have arrived at the temperature of 40° .

When fresh water, in cooling, has arrived at this temperature, it ceases to be farther condensed with cold, and its internal motions (which, as we have already more than once observed, are caused *solely* by the changes produced in the specific gravity of its particles) cease of course, and ice immediately begins to be formed on its surface; but as the condensation of salt water goes on as its Heat goes on to be diminished, its internal motions will continue; and it is evidently impossible for ice to be formed at its surface till the whole mass of the water has become ice-cold, or till its temperature is brought down to 32° . It would therefore give off a quantity of Heat equal to 8 degrees, at least, of Fahrenheit's thermometer, *more than the fresh water* would part with before ice could be formed on its surface.

To be able to form an idea of this enormous quantity of Heat, we have only to recollect what has already been said, and we shall find reason to conclude that it would be sufficient to melt a covering of ice equal in thickness to $\frac{2}{3}$ of the depth of the

the sea.—It would therefore be sufficient in that part of the North Sea (lat. 67°) where Lord Mulgrave founded at the depth of 4680 feet, to melt a cake of ice 265 feet thick!

But the Heat evolved in the formation of each superficial foot of ice would be sufficient to raise the temperature of a stratum of incumbent air 2220 times as thick as the ice, (consequently in the case in question 265×2220 feet, or 869 miles thick,) 28 degrees, or from the temperature of freezing water, to that of 50° of Fahrenheit's thermometer, or to the mean annual temperature of the northern parts of Germany!

The Heat given off to the air by each superficial foot of water in cooling *one degree* is sufficient to heat an incumbent stratum of air 44 times as thick as the depth of the water, 10 degrees. Hence we see how very powerfully the water of the ocean, which is never frozen over, except in very high latitudes, must contribute to warm the cold air which flows in from the polar regions.

But the ocean is not more useful in moderating the extreme cold of the polar regions, than it is in tempering the excessive heats of the torrid zone;—and what is very remarkable, the fitness of the sea water to serve this last important purpose is owing to the very same cause which renders it so peculiarly well adapted for communicating Heat to the cold atmosphere in high latitudes, namely, *to the salt which it holds in solution.*

As the condensation of salt water with cold continues to go on even long after it has been cooled to the temperature at which fresh water freezes, those particles

particles at the surface which are cooled by an immediate contact with the cold winds must descend, and take their places at the bottom of the sea, where they must remain, till, by acquiring an additional quantity of Heat, their specific gravity is again diminished. But this Heat *they never can regain in the polar regions*, for innumerable experiments have proved beyond all possibility of doubt, that there is no *principle of Heat* in the *interior parts of the globe*, which, by exhaling through the bottom of the ocean, could communicate Heat to the water which rests upon it.

It has been found that the temperature of the earth at great depth under the surface is different in different latitudes, and there is no doubt but this is also the case with respect to the temperature at the bottom of the sea, in as far as it is not influenced by the currents which flow over it; and this proves to a demonstration that the Heat which we find to exist, without any sensible change during summer and winter, at great depths, is owing to the action of the sun, and not to *central fires*, as some have too hastily concluded.

But if the water of the ocean, which, on being deprived of a great part of its Heat by cold winds, descends to the bottom of the sea, cannot be warmed *where it descends*, as its specific gravity is greater than that of water at the same depth in warmer latitudes, it will immediately begin to spread on the bottom of the sea, and to flow towards the equator, and this must necessarily produce a current at the surface in an opposite direction;

tion; and there are the most indubitable proofs of the existence of both these currents.

The proof of the existence of one of them would indeed have been quite sufficient to have proved the existence of both, for one of them could not possibly exist without the other; but there are several direct proofs of the existence of each of them.

What has been called the gulph stream, in the Atlantic Ocean, is no other than one of these currents that at the surface which moves from the equator towards the north pole, modified by the trade winds, and by the form of the continent of North America; and the progress of the lower current may be considered as proved directly by the cold which has been found to exist in the sea at great depths in warm latitudes;—a degree of temperature much below the mean annual temperature of the earth in the latitudes where it has been found, and which of course must have been *brought from colder latitudes*.

The mean annual temperature in the latitude of 67° has been determined by Mr. KIRWAN, in his excellent treatise on the temperature of different latitudes, to be 39° ; but Lord Mulgrave found on the 20th of June, when the temperature of the air was $48\frac{1}{2}^{\circ}$, that the temperature of the sea at the depth of 4680 feet was 6 degrees below freezing, or 26° of Fahrenheit's thermometer.

On the 31st of August, in the latitude of 69° , where the annual temperature is about 38° , the temperature of the sea at the depth of 4038 feet was

was 32° ; the temperature of the atmosphere (and probably that of the water at the surface of the sea) being at the same time at $59\frac{1}{2}^{\circ}$.

But a still more striking, and I might, I believe, say an incontrovertible proof of the existence of currents of cold water at the bottom of the sea, setting from the poles towards the equator, is the very remarkable difference that has been found to subsist between the temperature of the sea at the surface and at great depth, at the tropic,—though the temperature of the atmosphere there is so constant that the greatest changes produced in it by the seasons seldom amounts to more than five or six degrees; yet the difference between the Heat of the water at the surface of the sea, and that at the depth of 3600 feet, has been found to amount to no less than 31 degrees; the temperature above or at the surface being 84° , and at the given depth below no more than 53° *.

It appears to me to be extremely difficult, if not quite impossible, to account for this degree of cold at the bottom of the sea in the torrid zone, on any other supposition than that of cold currents from the poles; and the utility of these currents in tempering the excessive heats of those climates is too evident to require any illustration.

These currents are produced, as we have already seen, in consequence of the difference in the specific gravity of the sea water at different temperatures; their velocities must therefore be in pro-

* Phil. Transactions 1752.

portion to the change produced in the specific gravity of water by any given change of temperature; and hence we see how much greater they must be in salt water than they could possibly have been had the ocean been composed of fresh water.

It is not a little remarkable that the water of all great lakes is fresh, and nearly so in all inland seas (like the Baltic) in cold climates, and which communicate with the ocean by narrow channels. We shall find reason to conclude that this did not happen without design, when we consider what consequences would probably ensue should the waters of a large lake in an inland situation, in a cold country, (such as the lake Superior, for instance, in North America,) become as salt as the sea.

Though the cold winds which blow over the lake in the beginning of winter would be more warmed, and the temperature of the air on the side of the lake opposite to the quarter from whence these winds arrive, would be rendered somewhat milder than it now is; yet, as the water of the lake would give off an immense quantity of Heat before a covering of ice could be formed on its surface for its protection, it would, on the return of spring, be found to be *extremely cold*; and as it would require a long time to regain from the influence of the returning sun the enormous quantity of Heat lost during the winter, it would remain very cold during the spring, and probably during the greatest part of the summer; and this could not fail to chill the atmosphere, and check vegetation in the surrounding

ing

ing country to a very considerable distance. And though a large lake of salt water in a cold country would tend to render the winter *somewhat milder* on one side of it, namely, on the side opposite to the quarter from whence the cold winds came; yet this advantage would not only be confined to a small tract of country, but would not any where be very important, and would by no means counterbalance the extensive and fatal consequences which would be produced in summer by so large a collection of very cold water.

When the winter is once fairly set in,—when the earth is well covered with snow, and the rivers and lakes with ice, and more especially when the ice as well as the land is covered with that warm winter garment, a few degrees more of cold in the air cannot produce any lasting bad consequences. It may oblige the inhabitants to use additional precautions to guard themselves, their domestic animals, and their provisions, from the uncommon severity of the weather; but it can have very little influence in the temperature of the ensuing summer; and even it is probable, if it influences it at all, that it tends rather to make it *warmer* than *colder*. Lakes of salt water could therefore be of no real use *in winter* in cold countries, and in summer they could not fail to be very hurtful; while fresh lakes, as they are frozen over almost as soon as the winter sets in, and long before the whole mass of their water is cooled down to the temperature of freezing, they preserve the greater part of their

their Heat through the winter, and if they are of no use during the cold season, they probably do little or no harm in summer.

But I must take care not to tire my reader by pursuing these speculations too far. If I have persisted in them,—if I have dwelt on them with peculiar satisfaction and complacency,—it is because I think them uncommonly interesting,—and also because I conceived that they might be of real use in this age of *refinement* and *scepticism*.

If, among barbarous nations, the *fear of a God*, and the practice of religious duties, tend to soften savage dispositions, and to prepare the mind for all those sweet enjoyments which result from peace, order, industry, and friendly intercourse,—a *belief in the existence of a Supreme Intelligence*, who rules and governs the universe with wisdom and goodness, is not less essential to the happiness of those who, by cultivating their mental powers, **HAVE LEARNED TO KNOW HOW LITTLE CAN BE KNOWN.**

DESCRIP.

Fig. 1

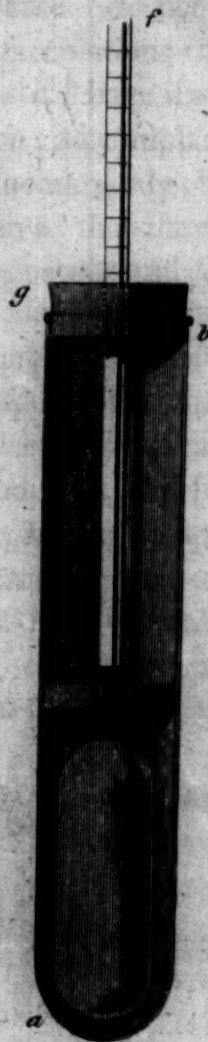
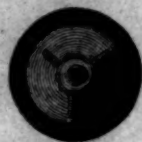
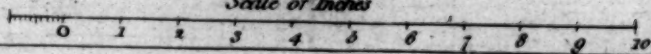


Fig. 2



Scale of Inches



Scale of Strand

DESCRIPTION OF THE PLATES.

PLATE I.

THIS Plate represents the cylindrical Passage Thermometer used in the Experiments, on the conducting power of liquids with regard to Heat.

Fig 1. *a, b*, is a section of the brass tube in which the Thermometer *c*, with an oblong copper bulb, is placed.

e, f, is the glass tube of the Thermometer, which, for want of room in the Plate, is represented as broken off at *f*.

g, is a stopple of cork by which the end of the brass tube, *a, b*, is closed; and

h, is a circular disk of the same substance.

The space in the brass tube below this disk *h*, surrounding the bulb of the Thermometer, was occupied by the liquid whose conducting power was determined. The space between the disk and the cork stopper *g*, was filled with eider-down.

Between the inside of the brass tube and the lower part of the bulb of the Thermometer are seen the wooden pins which served to confine the Thermometer in its place.

Fig. 2. This is an horizontal section of the brass tube, and a bird's-eye view of the Thermometer in its place.

PLATE II.

Fig. 3. This Figure shows the manner in which the Experiments were made, in which a cake of ice at the bottom of a tall glass jar was thawed by hot water standing on its surface.

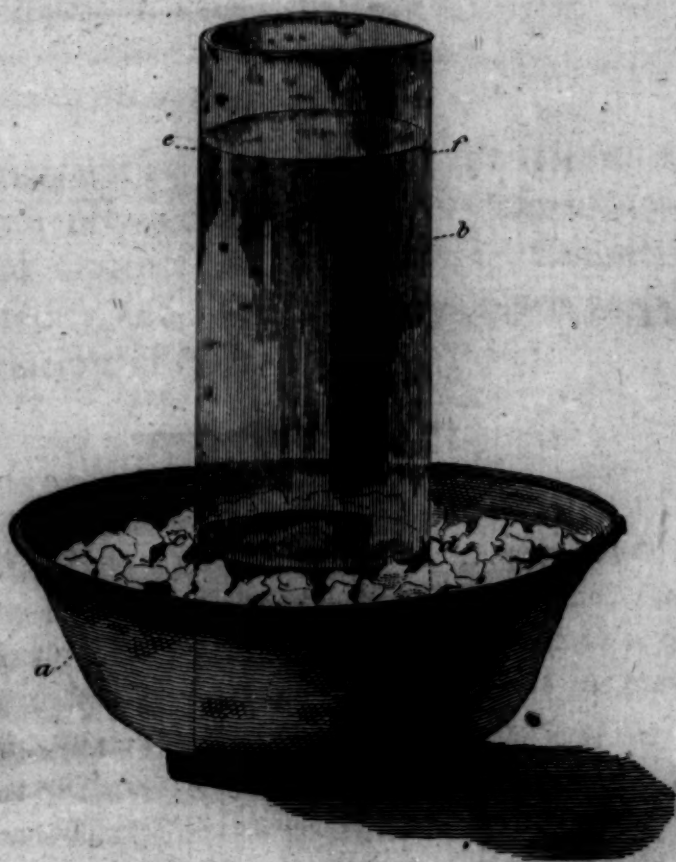
a, is an earthen bowl filled with pounded ice and water, in which the glass jar, *b*, was placed.

c, d, is the level of the upper surface of the ice in the jar.

e, f, is the level of the surface of the water standing on the ice in the jar.

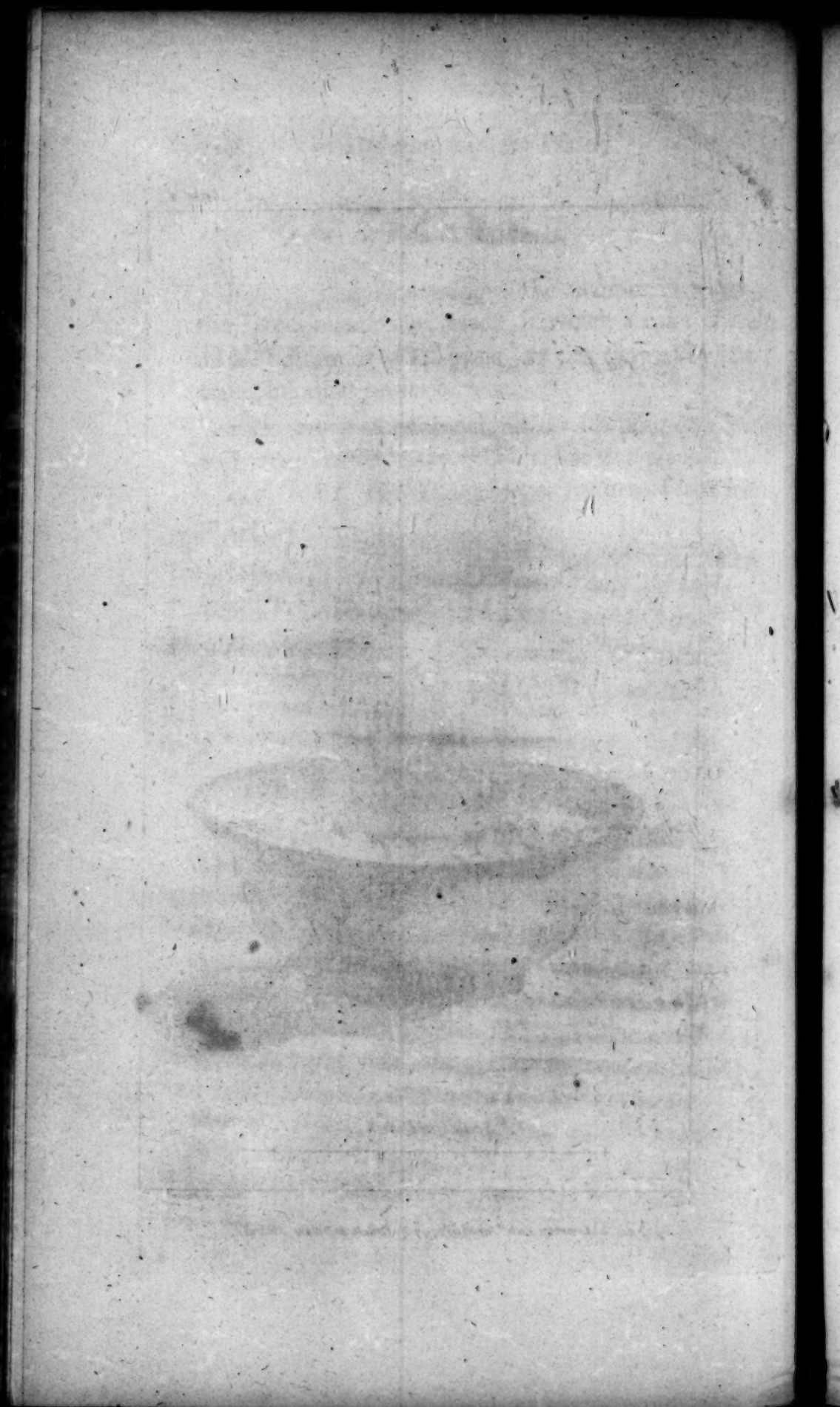
END OF THE SEVENTH ESSAY.

Fig. 3



Note on Strand.

London Pub.^d June 10. 1797 by Cadell & Davies Strand.



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PART II.

An Account of several NEW EXPERIMENTS,
with occasional Remarks and Observations,
and CONJECTURES respecting Chemical
Affinity and Solution, and the mechanical
Principle of Animal Life.

CHAP. I.

*ACCOUNT of a Circumstance of a private Nature,
by which the Author has been induced to add this
and the following Chapters to the Second Edition of
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ject continued.—OIL found by Experiment to be a
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that Fact may be of great Use in enabling us to form
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ESSAY VII.

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traction of Affinity.—Strata of fresh Water and of salt Water may be made to repose on each other in actual Contact without mixing.—Probability that the Water at the Bottom of fresh Lakes, that are very deep, may be actually salt.

AT the end of a French translation of the First Edition of this Essay, published at Geneva, Professor Pictet (the translator) has added the following extract of one of my private letters to him, (of the 9th June 1797,) written in answer to one from him to me, acknowledging the receipt of a manuscript copy of the Essay which I had sent him.

“ I should have been much surprised if my Seventh Essay had not interested you ; for in my life I never felt pleasure equal to that I enjoyed in making the experiments of which I have given an account in that performance. You will perhaps be surprised when I tell you, that I have suppressed a whole Chapter of interesting speculation, merely with a view of leaving to others a tempting field of curious investigation *untouched*, and to give more effect to my concluding reflection, which I consider as being by far the most important of any I have ever published.”

As these assertions, (which were not originally intended for the public eye,)—are liable to several interpretations, I think it my duty, not only to explain them, but also to let the Public know precisely how far I have pushed my inquiries in the investigation of the subject under consideration : This is an act of justice which I owe to those who may be engaged

engaged in the same pursuits; for it would be very unfair, by *obscure hints of important information kept back*, to keep others in doubt with respect to the originality of the discoveries they may make in the prosecution of *their* investigations. This would tend to *damp* the spirit of inquiry, instead of *exciting* it; and throwing out such hints looks so much like lying in wait to seize on the fair fruits of the labours of others, that I cannot rest till I have shewn that I do not deserve to be suspected of such pitiful views.

My worthy friend, Professor Piçtet, certainly did not suspect any unhandsome design in any thing I said to him in my (private) letter; but those who are less acquainted with my character, may not be disposed to give me credit for candour and disinterestedness without proofs.

With regard to the assertion in my letter, “that I had suppressed a whole Chapter of interesting speculation, with a view to leaving to others a tempting field, *untouched*, for curious investigation;”—this is perfectly true in fact, as will, I flatter myself, appear, by what I shall now lay before the Public; and I am confident that those who will take the trouble to consider with attention the reasons which induced me to do this, will find them such as will deserve their approbation:

Having, as I flattered myself, laid open a new and most enticing prospect to those who are fond of philosophical pursuits, I was afraid, if I advanced too far, that others, instead of striking out roads for themselves, might perhaps content themselves with

following in my footsteps ; and consequently that many, and probably the most interesting parts of the new field of enquiry, would remain a long time unexplored : And with regard to the reputation of being a *discoverer*, though I rejoice, I might say, exult and triumph—in the progress of human knowledge,—and enjoy the sweetest delight in contemplating the advantages to mankind which are derived from the introduction of useful improvements ; yet I can truly say, that I set no very high value on the honour of being the first to stumble on those treasures which every where lie so slightly covered.

In respect to the “ concluding reflection ” of the First Edition of this Essay ;—though some may smile in pity, and others frown at it, I am neither ashamed nor afraid to own, that I consider the subject as being of the *utmost importance* to the peace, order, and happiness of mankind, *in our present advanced state of society*. But to return from these digressions—

Though it appeared to me that the important fact I undertook to investigate, relative to the *manner* in which Heat is propagated in Fluids, is fully established by the Experiments, of which an account has been given in the preceding Chapters of this Essay ; yet, as a thorough examination of the subject is a matter of much importance, in many respects, I did not rest my enquiries here, but made a number of Experiments with a view to throwing still more light upon it, and enabling us to form more clear and distinct ideas respecting those curious mechanical

mechanical operations which appear to take place in Fluids, when Heat is propagated in them.

Having frequently observed when a quantity of water in one of my glass jars was frozen to a cake of ice, by placing the jar in a freezing mixture, that, as the ice first began to be formed at the sides of the jar, and increased gradually in thickness, the portion of water in the axis of the jar (which last retained its fluidity) being compressed by the expansion of the ice, was always forced upwards towards the end of the process, and formed a pointed projection of ice in the form of a nipple, (*papilla*), which was sometimes above half an inch high in the middle of the upper side of the cake; I was led by that circumstance to make the following interesting Experiments.

Experiment, No. 55.

A cake of ice, 3 inches thick, which had a pointed projection, $\frac{1}{2}$ an inch high, which arose from the centre of its upper surface, being frozen fast in the bottom of a tall cylindrical glass jar, $4\frac{3}{4}$ inches in diameter; this jar, standing in an earthen pan, and being surrounded by pounded ice and water, to the height of an inch above the level of the upper surface of the cake of ice, was placed on a table, near a window, in a room where the air was at the temperature of 31° of Fahrenheit's thermometer; and fine *olive oil*, which had previously been cooled down to the temperature of 32° , was

poured into the jar till it stood at the height of 3 inches above the surface of the cake of ice.

Having ready a solid cylinder of wrought iron, $1\frac{1}{4}$ inch in diameter, and 12 inches long, with a small hook at one end of it, by means of which it could occasionally be suspended in a vertical position, and furnished with a fit hollow cylindrical sheath of thick paper, into which it just passed,—open at both ends, and about $\frac{1}{16}$ of an inch longer than the solid cylinder of iron, to which it served as a covering for keeping it warm; this iron cylinder, being heated to the temperature of 210° in boiling water, and being suddenly introduced into its sheath, was suspended by an iron wire which descended from the ceiling of the room, in such a manner, that its lower end entering the jar, (in the direction of its axis,) was immersed in the oil to such a depth, that the middle of the flat surface of this end of the hot iron, which was directly above the point of the conical projection of ice, was distant from it only $\frac{2}{16}$ of an inch. The end of the sheath descended $\frac{1}{16}$ of an inch lower than the end of the hot metallic cylinder.

As the oil was very transparent, and the jar placed in a favourable light, the conical projection of ice was perfectly visible, even after the hot cylinder was introduced into the jar; and had *any Heat* DESCENDED through the thin stratum of fluid oil which remained interposed between the hot surface of the iron and the pointed projection of ice, which was under it, there is no doubt but this Heat must have been apparent, by the melting of
the

the ice; which event would have been discovered, either by the diminution of the height of this projection, or by an alteration of its form. But this was not the case: the ice did not appear to be in the smallest degree diminished, or otherwise affected by the vicinity of the hot iron.

My reader will naturally suppose, without my mentioning the circumstance, that due care was taken in introducing the cylinder into the jar, to do it in the most gentle manner possible, to prevent the oil from being thrown into undulatory motions; and that proper means were used for confining the cylinder, motionless, in its place, when it had arrived there.

As this experiment appears to me to be unexceptionable, and its result unequivocal and decisive; in order that a perfect idea may the more easily be formed of it, I have added the Figure 4, where a section of the whole of the apparatus used in making it may be seen, expressed in a clear and distinct manner.

If the general result of the Experiments, of which an account has been given in the two first Chapters of this Essay, afforded reason to conclude that *water* is a *non-conductor* of Heat, the result of that here described certainly proves, in a manner quite as satisfactory, that *oil* is also a *non-conductor*; and serves to give an additional degree of probability to the conjecture, that all Fluids are *necessarily* non-conductors of Heat.

As *mercury*, which is a metal in fusion, is different in many respects from all other Fluids, I

was very impatient to know if it agreed with them in that essential property, from which they have been denominated non-conductors of Heat, and this I found to be actually the case, by the result of the following decisive Experiment.

Experiment, No. 56.

Having emptied and cleaned out the cylindrical glass jar used in the last mentioned Experiment, and replenished it with a fresh cake of ice, with a conical projection in the middle of its upper side, I placed the jar, surrounded by pounded ice and water, on the table, in the cold room, where the foregoing Experiment had been made; and poured over the cake of ice as much ice-cold mercury, as covered it to the height of about an inch. Having cleaned the surface of the mercury in the jar with blotting paper, I suffered the whole to remain quiet, about an hour; and then very gently introduced the end of the hot cylinder of iron (inclosed in its paper sheath) into the mercury, and fixed it immovably in such a position, that its flat end, which was naked, was immediately over the point of the conical projection of ice, and distant from it about $\frac{1}{4}$ of an inch; where I suffered it to remain several minutes.

It is necessary that I should mention, that, in order to prevent the internal motions in the mass of mercury, which would otherwise have been occasioned by the rising and spreading out on its surface of those particles of that fluid, which, having
touched

touched the flat end of the hot iron, became specifically lighter in consequence of their increase of temperature, the end of the hollow cylindrical sheath, in which the solid cylinder of iron was placed, was made to project about $\frac{1}{10}$ of an inch below the flat end of the iron. This precaution was likewise used, and for a similar reason, in the preceding Experiment; when oil was used in the place of the mercury; as was mentioned, though without being explained, in giving an account of that Experiment.

As the cake of ice, on which the mercury reposed, was at that temperature precisely at which ice is disposed to melt with the smallest additional quantity of Heat, if any Heat had found its way *downwards* through the mercury to the ice in this Experiment, water would most undoubtedly have been formed; and this water would as undoubtedly have appeared on the surface of the mercury on taking away the iron: but there was not the smallest appearance of any ice having been melted.

To find out whether the cake of ice was *really* at that temperature at which it was disposed to melt with any additional Heat, I thrust down the end of my finger through the mercury, and touched the ice; and this Experiment removed all my doubts; for I found that, however expeditiously I performed that operation, it was hardly possible for me to touch the ice without evident signs of water having been produced being left behind, on the clean and bright surface of the mercury, on taking away my finger.

From

From the results of all these experimental investigations it appears to me, that we may safely conclude that *water, oil, and mercury* are perfect *non-conductors* of Heat; or, that when either of those substances takes the form of a Fluid, all interchange and communication of Heat *among its particles*, or from one of them to the other, directly, becomes from that moment *absolutely impossible*.

That this is also the case with respect to the particles of *air*, has been rendered extremely probable,—I believe I might say proved,—by the Experiments of which I gave an account in one of my Papers on Heat, published in the Transactions of the Royal Society;—and I have shewn elsewhere—(in my Sixth Essay) how much reason there is to conclude, that the particles of *Steam* and of *Flame* are in the same predicament.

But if all interchange and communication of Heat, from particle to particle—*immediately, or de proche en proche*, be absolutely impossible in so many *elastic and unelastic Fluids*,—and in Fluids so essentially different in many other respects,—is there not sufficient grounds to conclude from hence, that this property is common to all Fluids—and that it is even *essential to fluidity*?

It is easy to conceive, that the discovery of so important a circumstance must necessarily occasion a considerable change in the ideas we have formed in respect to the mechanical operations which take place in many of the great phenomena of Nature; as well as in many of those still more interesting
chemical

chemical operations, which we are able to direct, but which we find, alas ! very difficult to explain.

In my Paper on Heat, above mentioned, published in the Philosophical Transactions for the year 1792, I endeavoured to apply the discovery of the non-conducting power of *air* in accounting for the warmth of the hair of beasts ;—of the feathers of birds ;—of artificial clothing ;—and of snow, the winter garment of the earth ;—and also, in explaining the causes of the cold winds from the polar regions, and of their different directions in different countries, which prevail at the end of winter, and early in the spring.

In my Sixth Essay—(on the Management of Heat and the Economy of Fuel)—I availed myself of the knowledge of the non-conducting power of *steam* and of *flame*, in explaining the effects of a blow-pipe in increasing the action of pure flame ; and in investigating the most advantageous forms for boilers : and in the third Chapter of this Essay I have endeavoured to apply the discoveries which had been made, respecting the manner in which Heat is propagated in *water*, in explaining the means which appear to have been used by the Creator of the world for equalizing the temperatures of the different climates, and preventing the fatal effects of the extremes of heat and of cold on the surface of the globe. But a most interesting application remains to be made of these discoveries, to *chemistry* ;—*vegetation* ;—and the *animal economy* ;—and to the learned in those branches of science I beg leave most earnestly to recommend them. If I am not
much

much mistaken they will throw a new light on many of those mysterious operations of Nature, in which *inanimate bodies* are put in motion—their forms changed—their component parts separated, and new combinations formed; and it is possible that they may even enable us to account, on mechanical principles, for those surprising appearances of preference and predilection among bodies, which, without ever having been attempted to be explained, have been distinguished by the appellation of *chemical affinity*.

Perhaps it will be found that every change of form, in every kind of substance, is owing to Heat; and to Heat alone:—that every concretion is a true *congelation*, effected by cold, or a diminution of Heat;—and that every change from a solid to a fluid form is a real *fusion*. That the difference between calcination in the *wet* and in the *dry* way, is, in fact, much less than has hitherto been generally imagined; and that no metal is ever dissolved till it has *first been melted*.

Perhaps it will be found, that the apparent violence with which solid bodies of some kinds are attacked by their liquid solvents,—and which has, I believe, been considered as a proof of a strong chemical affinity—is not owing to any particular attraction, or election, but to the considerable degree of heat, or of cold, which is produced in their union with their *menstrua*; or to a great difference in the specific gravity of the *menstruum* in its natural state, and that of the same fluid after it has been changed to a saturated solution.

If

If Fluids are non-conductors of Heat, it is evident that, if any change of temperature takes place in chemical solution, it must necessarily produce *currents* in the solvent; and that these currents must be the more rapid, as the change of temperature is greater; and as they necessarily cause a succession of fresh particles of the solvent to come into contact with the solid, it is evident,—all other things being equal,—that the rapidity of the process of solution will be as the rapidity of these currents,—or as the change of temperature.

But the currents produced by the difference in the specific gravities of the fluid menstruum, and of the saturated solution, have perhaps, in general, a still greater effect in bringing a rapid succession of fresh particles of the menstruum into contact with the solid body that is dissolved in it, than those produced by the change of temperature.

When these two causes conspire to accelerate the motion of the same current, or when their tendencies are *in the same direction*, as is the case in the solution of common sea-salt in water,—the solution ought to be most rapid.

When common salt is dissolved in water, the specific gravity of the saturated solution is greater than that of pure water, and will therefore descend in it; and cold being produced in the process, and water being a non-conductor of Heat, the specific gravity of the saturated solution will be *still farther increased*, in consequence of its condensation with this cold, by which its descent in the water will be still farther accelerated.

A curious question here presents itself, which, could it be resolved, might greatly tend to elucidate this abstruse subject of philosophical investigation. Supposing that, in a case where Heat is generated in the solution of a solid in a fluid menstruum, the *addition* to the specific gravity of the menstruum, arising from its chemical union with the solid, should so precisely counter-balance the *diminution* of the specific gravity of the Fluid, by the Heat generated in the process, that the *hot* saturated solution should be precisely of the same specific gravity as the *cold* menstruum;—would, or would not the process of solution be possible under such circumstances?

If the *apparent* tendency to approach each other, which we sometimes perceive in solids and their fluid menstrea, were real;—if that peculiar kind of attraction of predilection which has been called chemical affinity, has a real existence, and if its influence reaches *beyond the point of actual contact*, (as has, I believe, been generally supposed,) as there is no appearance of any attraction whatever, or affinity, between any solid body, and a saturated solution of the same body in its proper menstruum, it seems probable that the solution would take place, —under the circumstances described: but should the attraction of affinity, according to the definition of it here given, have no existence in fact, (which is what I very much suspect,) in that case it is evident that the solution, though it would not be absolutely impossible, would be so very slow as hardly to be perceptible.

It

It would not be *impossible*, because the particles of the menstruum in immediate contact with the solid, though, in the moment of their saturation, they would have no tendency to move out of their places, yet, as they would by degrees necessarily give off, to the undissolved part of the solid, a part of the Heat acquired in the chemical process by which they were saturated, being condensed by this loss of Heat, they would, at length, begin to descend, and give place to other particles of the menstruum; which, in their turns, would follow them, but with velocities, however, continually decreasing;—on account of the gradual augmentation of temperature of the undissolved part of the solid, and of the Heat communicated by that solid substance to the whole mass of the liquid menstruum.

Though it would, probably, be extremely difficult to contrive any single experiment, from the result of which a satisfactory decision of this question could be obtained, yet it does not appear to be impossible to discover by *indirect means*, the principal fact on which its decision must depend.

It is a well known fact, that, when water which holds sea-salt in solution is mixed, in any vessel, with fresh water, the salt will, after a short time, be found to be very equally distributed in every part of the whole mass; and I believe that it has been generally considered, that this equal distribution of the salt is owing to the affinity which is supposed to exist between sea-salt and water.

Having doubts with respect to the existence of this supposed attraction; and suspecting that the

equal distribution of the salt was owing to a very different cause—the internal motions among the particles of the water, occasioned by accidental changes of temperature—I made the following Experiment, which, I fancy, will be considered as decisive.

Experiment, No. 57.

I took a cylindrical glass jar, $4\frac{1}{4}$ inches in diameter, and $7\frac{3}{4}$ inches high, and placing it in the middle of another cylindrical glass jar, $7\frac{1}{2}$ inches in diameter, and 8 inches high, which stood in a very shallow earthen dish, nearly filled with pounded ice and water, I placed the dish, with its contents, on a strong table, in an uninhabited room, in a retired part of the house, where the temperature of the air, which was the same, with very little variation, day and night, was at about 36° F. Having prepared, and at hand, a quantity of the strongest *brine* I could make with sea-salt, which was very clear, transparent, perfectly colourless, and ice-cold; and also, a quantity of fresh, or pure water,—ice-cold, lightly tinged of a red colour with turnsol; and some ice-cold *olive-oil*; I first poured as much of the fresh water into the small cylindrical jar as was necessary to fill it up to the height of above 2 inches; and then, by means of a glass funnel, which ended in a long and narrow tube, by introducing this tube into the fresh water, and resting it on the bottom of the jar, I poured a quantity

quantity of the brine, equal to that of the fresh water, into the jar; and in performing this operation I took so much care to do it gently, and without disturbing the fresh water already in the jar, that, when it was finished, the fresh water, which, as it was coloured red, could easily be distinguished from the brine, remained perfectly separated from this heavier saline liquor; on which it reposed quietly, without the smallest appearance of any tendency to mix with it.

I now filled, to the height of about 5 inches, the void space between the outside of the small jar and the inside of the large jar in which it was placed, with ice-cold water, mixed with a quantity of ice, in pieces as large as walnuts,—(pounded ice would have obstructed the view in observing, through the sides of the large jar, what passed in the smaller)—and when this was done, I very carefully poured ice-cold *olive oil* * into the smaller jar till it covered the surface of the (tinged) fresh water to the height of about an inch (see Fig. 5. Plate IV.); and placing myself near the table, in a situation where I had a distinct view of the contents of the small jar, I set myself to observe the result of the Experiment.

After waiting above an hour without being able to perceive the smallest appearance of any motion, either in the brine, or in the fresh water, (the one continuing to repose on the other with the most

* This oil served not only to keep the water on which it reposed, quiet; but also to prevent any communication of heat between it and the air of the atmosphere.

perfect tranquillity, and without the smallest disposition to mix together) I left the room.

When I returned to it the next day, I found things precisely in the state in which I had left them; and they continued in this state, without the smallest appearance of any change, or of any disposition to change, during *four days*.

At the end of that time, thinking that any farther prolongation of the Experiment would be quite useless, I removed the small jar, taking care not to agitate its contents, and placed it in the window of a room heated by a German stove.

In less than an hour I perceived that the brine and the (tinged) fresh water began to mix, and at the end of 24 hours they were intimately mixed throughout, as was evident by the colour of the aqueous fluid on which the oil reposed; which now appeared to the eye to form one uniform mass of a light red tint.

I shall leave it to philosophers to draw their own conclusions from the results of this Experiment: In the mean time, there is one fact which it seems to point out that I shall just mention, which is not only curious in itself, but may lead to very important discoveries. It appears to me to afford strong reasons to conclude that, were a lake but *very deep*, its waters, near the surface, would necessarily be fresh, even though its bottom should be one solid mass of rock salt!

Would it be ridiculous to make Experiments to determine whether the water at the bottom of some very deep lakes is not impregnated with salt?

Should it be found to be actually the case, it might prove an unexhaustible treasure in an inland country, where salt is scarce.

As mines of rock-salt are often found in the neighbourhood of fresh lakes, it seems reasonable to suppose that the waters of such lakes should *sometimes* be in contact with *strata* of these mines; and when I first began to meditate on this subject, I was much surprised,—not that the salt water which may lie at the bottom of fresh lakes should not already have been discovered,—for from the first I plainly perceived that nothing could happen in the ordinary course of things that could bring it to the light, or even afford any grounds to suspect its existence;—but, as *strata* of salt mines frequently lie higher than the mean level of the country, I was surprised that lakes of salt water should not more frequently be found; and as these reflections occurred to me *after* I had discovered what appeared to me to be an evident proof of the wisdom and goodness of the Creator in *making* all lakes in cold countries *fresh*, I began to be alarmed for the fatal consequences that might ensue, if, by chance, the side of a lake should come into contact with a mountain of salt; as I saw might easily happen.

Shall I,—or shall I not attempt to give my reader an idea of what I felt, when, meditating on the subject, and almost beginning to repent of what many, no doubt, have already condemned as the foolish dream of an enthusiastic imagination, I saw, all at once, that the most effectual care had been taken to prevent the evils I apprehended;—that

from the very constitution of things, and the ordinary and uniform operation of the known laws of Nature, the permanent *existence of a lake*, SALT AT THE SURFACE, is *absolutely impossible*; even though it should be surrounded on every side by mountains of salt *?

Though the explosion of a volcano, an earthquake, or any other great convulsion, by which the shores of a lake might be brought into contact with a vast mine of salt, might cause the whole mass of its water to be salt for a time; yet, the evil would soon effect its own remedy: The falling in of the crust of earth and stones by which mines of salt are every where found to be covered, (and without which they could not exist) would very soon cover the naked salt, and the water *at the surface of the lake* would again become perfectly fresh. Should, however, the lake be so deep that the temperature at the bottom should remain the same summer and winter, without any sensible variation, it is most certain that its waters *there*—(at the bottom of the lake)—would remain perfectly saturated with salt for ever.

But are there not some reasons to conclude that the water at the bottoms of *all very deep lakes* ought necessarily to be salt, even in situations where there are no mines of salt near?

The sea-shells that are frequently found in high inland situations, as well as many other appear-

* By the word *Lake* I mean, as is easy to perceive, a collection of water, in a high inland situation, from which there is a constant efflux.

ances noticed by naturalists, strongly indicate that most of our continents have been covered by the waters of the ocean. Now if that event ever happened—however remote the period may be at which it took place—it seems highly probable that the salt water left at the bottoms of all deep lakes, by the sea, on its retiring, *must be there now.*

I cannot take my leave of this subject without just observing, that the discovery of the *impossibility* of the permanent existence of what we can plainly perceive would be an evil, certainly ought not to *diminish* our admiration of the wisdom of the great Architect of the Universe.

CHAP. II.

Water made to congeal at its under Surface.—Observation respecting the Formation of Ice at the Bottoms of Rivers.—Reasons for concluding that Heat can never be equally distributed in any Fluid.—Perpetual Motions occasioned in Fluids by the unequal Distribution of Heat.—An inconceivably rapid Succession of Collisions among the integrant Particles of Fluids is occasioned by the internal Motions into which Fluids are thrown in the Propagation of Heat.—An Attempt to estimate the Number of those Collisions which take place in a given Time.—These Investigations will greatly change our Ideas respecting the real State of Fluids apparently at rest.—FLUIDITY may be called the LIFE OF INANIMATE BODIES.—Conjectures respecting the VITAL PRINCIPLE in Living Animals ; and the Nature of Physical STIMULATION.

WHATEVER the mechanical operation may in fact be, by which those effects are produced that have given rise to the idea of the existence of an attraction of affinity—(a power different from gravitation)—between solid bodies and their liquid menstrea, and between different portions of the same menstruum differently saturated ; the result of the foregoing Experiment (No. 57) proves that two particles of water in combination with very different

different quantities of sea-salt; or a particle of water *saturated* with salt, and another perfectly free from salt; *may be* in contact with each other for any length of time without showing any appearance of a disposition to equalize the salt between them.

But should we even admit as a fact, what this Experiment seems to indicate, namely, that there is no such thing as an *attraction of predilection* between solids and their solvents; and that all those motions which have been attributed to the action of that supposed power,—(as well as all other motions which take place in Fluids,)—are the immediate effects of *gravitation* acting according to immutable laws, and *changes of specific gravity by Heat*; yet, there would still remain one great difficulty in explaining chemical solution. As all mechanical operations require *a certain time* for their performance; and as the motion which is occasioned in a Fluid by a change of specific gravity in any individual particles of it, *begins* as soon as the change begins to take place, if there be no attraction between the particles of solid bodies and the particles of their menstrua;—as Heat is supposed to be generated or absorbed, or, to speak more properly,—both generated and absorbed,—in the *contact* of those particles, and previous to the completion of their chemical union;—how does it happen that the particle of the menstruum whose specific gravity is necessarily changed by this change of temperature, does not *immediately* quit the solid, in consequence of this change; and before the process of solution has *had time to be completed*?

A consideration of the effects of the *vis inertiae* of the particle of the menstruum whose specific gravity is thus changed, and also of the *vis inertiae* of the rest of the Fluid, and the resistance it must oppose to the motion of its individual solitary particles, would furnish us with arguments that might be employed with advantage in removing this difficulty; but I fancy that the result of the Experiment of which I shall presently give an account will be more satisfactory than any reasoning, unsupported by facts, that I could offer on the subject.

When a doubt arises with regard to the *possibility* of any operation of a peculiar kind, which is *supposed* to take place, in any process of nature among those infinitely small integrant particles of bodies which escape, and must ever escape, the cognizance of our gross organs, however they may be assisted by art, the shortest way of deciding the question is to put the known powers of nature in action under such circumstances that the effects produced by them must show, unequivocally, whether the supposed operation be possible, or not: and if it be found to be possible in one case, we may then argue with less diffidence on the probability of its actually taking place in the specific case in question.

It has been abundantly proved by the Experiments of M. de Luc, and by those of my friend Sir Charles Blagden, that, when water, in cooling, has arrived at the temperature of about 41° F. its condensation with cold ceases; and it begins to expand;

expand; and continues to expand gradually as its temperature goes on to be farther diminished, till it is changed to ice. Availing myself of that most important discovery, I made the following Experiment,

Experiment, No. 58.

Having poured *mercury*, at the temperature of 60° , into a common glass-tumbler, till this Fluid stood at the height of about an inch; I then poured about twice as much water (at the same temperature) upon it; and placing the tumbler in a shallow earthen dish, surrounded it to the height of the level of the surface of the mercury with a freezing mixture of snow and common salt. Having done this, I was very curious indeed to see in what part of the water ice would first make its appearance. Could it be at the upper surface of it? That appeared to me to be impossible; for the Experiment being made in a room warmed by a German stove, the temperature of the air which reposed on that surface was considerably above the point at which water freezes.

Could it be at its lower surface, where it rested on the upper surface of the mercury?—If that should happen, it would show, that, notwithstanding the diminution of the specific gravity of the water in passing from the temperature of 41° to that of 32° ; and the tendency which this diminution gave it to quit the surface of the mercury from the instant when, in being cooled by a contact with it, it had passed the point of 41° ; yet, there

there was time sufficient for the congelation to be completed *before the particle of water so cooled could make its escape.*

The reader will naturally conclude from what was said in the preceding page, that it was merely with a view to the determination of that single fact, that this Experiment was contrived; and he will perceive by the result of it that my expectations with regard to it were fully answered.

Ice was not only formed *at the bottom of the water*, at its under surface, where it was in contact with the cold mercury; but, I found on repeating the experiment, and varying it, by previously cooling the mercury in the tumbler to about 10° , that *boiling hot water*, poured gently upon it, was instantly frozen, and gradually formed a thick cake of ice, covering the mercury; though almost the whole of the mass of the unfrozen water, which rested on this ice, remained nearly boiling hot.

This Experiment not only determines the point for the decision of which it was undertaken; but also enables us to form a just opinion respecting a matter of fact which has been the subject of a good deal of dispute.

Though many accounts have been published of ice found at the bottom of rivers, yet doubts have been entertained of the possibility of its being *formed* in that situation. From the result of the foregoing Experiment it appears to me that we may safely conclude, that, if after a very long and a very severe frost, by which the surface of the ground has not only been frozen to a considerable depth,
but

but also cooled several degrees below the freezing point, a river should overflow its banks, and cover the surface of ground *previously so cooled*, ice would be formed at the bottom of the water: but all the Experiments that have been made on the congelation of water shew the absolute impossibility of ice being ever formed, in any country, at the bottom of a river which constantly fills its banks, or which never leaves its bed exposed, dry, to the cold air of the atmosphere.

By reflecting on the various consequences that ought to follow from the peculiar manner in which Heat appears to be propagated in Fluids, we are led to conclude, that it is almost impossible that any Fluid exposed to the action of light should ever be throughout of the same temperature, though its mass be ever so small; and that the difference in the Heat of its different particles must occasion perpetual motions among them.

Suppose any open vessel,—as a common glass tumbler for instance,—containing a piece of money, a small pebble, or any other small solid opaque body, to be filled with water, and exposed in a window, or elsewhere, to the action of the sun's rays. As a ray of light cannot fail to generate Heat when and where it is stopped or absorbed, the rays, which, entering the water, and passing through it, impinge against the small solid opaque body at the bottom of the vessel, and are *there absorbed*, must necessarily generate a certain quantity of Heat; a part of which will penetrate into the interior parts of the solid, and a part of it will be commu-

communicated to those colder particles of the water which repose on its surface.

Let us suppose the quantity of Heat so communicated to one of the integrant particles of the water to be so small, that its effect in diminishing the specific gravity of the particle is but just sufficient to cause it to move upwards in the mass of the liquid with the very smallest degree of velocity that would be perceptible by our organs of sight, were the particle in motion large enough to be visible. This would be at the rate of about *one hundredth part of an inch* in a second.

This velocity, though it appears to us to be slow in the extreme, when we compare it with those motions that we perceive among the various bodies by which we are surrounded, yet, we shall be surprised when we find what a rapid succession of events it is capable of producing.

If we suppose the diameter of the integrant particles, or *molécules* of water, to be *one millionth part of an inch*—(and it is highly probable that they are even less—*)—in that case, it is most certain that an individual particle, moving on in a quiescent mass of that Fluid with the velocity in question, namely, at the rate of $\frac{1}{10000}$ part of an inch in 1 second, would run through a space equal to

* Leaf gold, such as is prepared and sold by the gold-beaters, is not *four times* as thick as the diameter here assumed for the integrant particles of water. These leaves of solid metal have been found by computation to be no more than $\frac{1}{100000}$ of an inch in thickness. How much less must be the diameter of the integrant particles of gold?

ten thousand times the length of its diameter in one second, and consequently, would come into contact with at least six hundred thousand different particles of water in that time.

Hence it appears how inconceivably short the time must be that an individual particle, in motion, of any Fluid, can remain in contact with any other individual particle, not in motion, against which it strikes in its progress, (however slow that progress may appear to us to be) through the quiescent mass of the Fluid!

Supposing the contact to last as long as the moving particle employs in passing through a space equal to the length of its diameter—which is evidently all that is possible; and more than is probable;—then, in the case just stated, the contact could not possibly last longer than $\frac{1}{10000}$ part of a second! This is the time which a cannon bullet, flying with its greatest velocity, (that of 1600 feet in a second) would employ in advancing 2 inches.

If the cannon bullet be a *nine pounder*, its diameter will be four inches; and if it move with a velocity of 1600 feet (=19200 inches) in a second, it will pass through a space just equal to 4800 times the length of its diameter in 1 second. But we have seen that a particle of water moving $\frac{1}{1000}$ of an inch in a second actually passes through a space equal to 10000 times the length of its diameter in that time: Hence it appears that *the velocity with which the moving body quits the spaces it occupies* is more than twice as great in the particle of water, as in the cannon bullet!

There

There is one more computation which may be of use in enabling us to form more just ideas of the subject under consideration,—and surely too much cannot be done to enlighten the mind, and assist the imagination, in our attempts to contemplate those invisible operations of nature which nothing but the sharpest ken of the intellectual eye will ever be able to detect and seize.

As succeeding events which fall under the cognizance of our senses cannot be distinguished if they happen oftener than about *ten times in a second* *, it appears that when a particle of water moves in a quiescent mass of that fluid at the rate of $\frac{1}{1000}$ part of an inch only, in one second, its succeeding collisions with the different particles, at rest, of that fluid, against which it strikes as it moves on, must be so inconceivably rapid that no less than *one thousand* of them must actually take place, *one after the other*, in the shortest space of time that is perceptible by the human mind †.

After

* This assertion, in as far, at least, as it relates to objects of sight, may be proved by the following easy experiment: Let a wheel, with any known number of spokes, be turned round its axis with such a velocity as shall be found necessary, in order that the spokes may disappear or become invisible.—From the velocity of the wheel, and the number of spokes in it, the fact will be decided.

† It probably will not escape the observation of my learned readers, that the velocity which I have here assigned to the single particle of water, moving upwards in that fluid in consequence of a change of its specific gravity by Heat, though apparently very small,—($\frac{1}{1000}$ part of an inch in a second)—is, however, most probably considerably greater, in fact, than any individual *solitary* particle of that fluid could possibly acquire, in the supposed circumstances, by any change of temperature, however great, owing to the resistance which
would

After we have patiently examined the result of these investigations, and the imagination has become *familiarized* with the contemplation of the interesting facts they present to it, how much will our ideas be changed with regard to the real state of fluids apparently at rest! They will then appear to us to be, what no doubt they really are in fact, an assemblage of an infinite number of infinitely small particles of matter moving continually, or without ceasing, and with inconceivable velocities.

We shall then consider fluidity as the *life of inanimate bodies*, and congelation as the *sleep of death*;—and we shall cease to ascribe active powers, or exertions of any kind, to dead *motionless* matter.

But what shall we think of the *vital principle* in living animals?—Does not their life also depend on the internal motions in *their* fluids, occasioned by an *unequal* distribution of heat?—And is not

would necessarily be opposed to its motion by the quiescent particles of the fluid. Aware of this objection; and being desirous of being prepared to meet it, I took some pains to compute, by the rules laid down by Sir ISAAC NEWTON in his *Principia*, book ii. sect. vii., what the greatest velocity is that a solitary particle of water (supposed to be $\frac{1}{1000000}$ of an inch in diameter) could possibly acquire by a given change of its specific gravity: And I found that if the specific gravity of water at the temperature of 32° F. be taken at 1.00082, and its specific gravity at 80° , at 0.99759, as lately determined by accurate experiments, then, a single particle of water at the temperature of 80° , situated in a quiescent mass of that fluid at 32° , the greatest velocity this hot particle could acquire in moving upwards in consequence of its comparative levity would be that of $\frac{1}{2033}$ part of an inch in 1 second. This is at the rate of about one inch and an half in 1 hour.—But it is evident, that when great numbers of particles unite and form currents, they will make their way through the quiescent fluid with greater facility, and consequently will move faster.

Stimulation,

stimulation, in all cases, the mere mechanical effect of the communication of Heat?

It is an opinion which we know to be as old as the days of MOSES, that *the life of an animal resides in its blood*; and it is highly probable that it dates from a period still more remote. It was lately revived by an anatomist and physiologist, (now no more,) who was eminently distinguished for sagacity; and it appears to me that the late discoveries respecting the manner in which Heat is propagated in Fluids tend greatly to elucidate the subject, and to give to the hypothesis a high degree of probability.

According to this hypothesis—(as it may now be explained)—every thing that increases the *inequality of the distribution* of the Heat in the mass of the blood—(even though it should not immediately augment its quantity)—ought to increase the intensity of those *actions* in which life consists. But are there not many striking proofs that this is the case in fact?

Do not *respiration*,—*digestion*,—and *insensible perspiration* all tend evidently—(that is to say, according to our assumed principles, with regard to the manner in which Heat is propagated in Fluids)—to *produce*, and to *perpetuate* this inequality of heat in the animal fluids? And do we not see what an immediate and powerful effect they have in increasing the intensity of the action of the powers of life?

If animal life depends essentially on those *internal* motions in the animal fluids,—which, as
has

has been shown,—are occasioned by the difference of the *specific gravities* of their integrant particles, or *molécules*, arising from their different temperatures;—in that case, it is evident that the *vital powers* would be strengthened, or their action increased, either by *heat* or by *cold* properly applied. But is not this found to be the case in fact? Does not the *dram of brandy* at St. Petersburg produce the same effects as the *draught of iced lemonade* at Naples, and by the same mechanical operation, but acting in opposite directions? And does not the *loss of Heat*, by insensible perspiration, contribute as efficaciously to the preservation of that *inequality of temperature* which is essential to life, as the *introduction of Heat* into the system in respiration?

Is not the sudden coagulation of blood, when drawn from a living animal, and are not all the other rapid changes that take place in it, evident proofs of an unequal distribution of Heat? And does not the *viscosity* of blood, as well as its perpetual motions in the vascular system, contribute very powerfully to the preservation of that inequality?

Are not the livid spots on the surface of the body, which indicate a beginning of mortification, produced in consequence of a separation, or *precipitation* of the heterogeneous particles of the animal Fluids, according to their specific gravities and individual temperatures, occasioned by rest, or an interruption of circulation? And may we not emphatically pronounce such Fluids to be dead?

Would not any liquid in which Heat were *equally distributed* be a *fatal poison* if injected into the veins of a living animal? And would not this be the case even were the liquid so injected a portion of the animal's own blood, or of the lymph or any other of its component parts, and were it at the mean temperature precisely of the healthy Fluids circulating in the veins and arteries of the animal?

Is not glandular secretion a true precipitation? and is it not possible that the formation of the solids, and the growth of an animal body, may be effected by a process exactly similar to congelation? And are there not even circumstances from which we might conclude, with a considerable degree of probability, that most of these congelations are formed at or about the temperature of boiling water?

But I forbear to enlarge on this subject. I find I have unawares entered a province, where, if I advance farther, I shall certainly be exposed to the danger of being considered and treated as an intruder; and I must hasten to make my retreat, which I shall endeavour to effect by abruptly putting an end to this Chapter.

2

CHAP. III.

Probability that intense Heat frequently exists in the solitary Particles of Fluids, which neither the Feeling nor the Thermometer can detect.—The Evaporation of Ice during the severest Frost explained on that Supposition.—Probability that the Metals would evaporate when exposed to the Action of the Sun's Rays were they not good Conductors of Heat.—Mercury is actually found to evaporate under the mean Temperature of the Atmosphere.—This Fact is a striking Proof that FLUID MERCURY is a Non-conductor of Heat.—Probability that the Heat generated by the Rays of Light is always the same in Intensity; and that those Effects which have been attributed to Light ought perhaps in all Cases to be ascribed to the Action of the Heat generated by them—A striking Proof that the most intense Heat does sometimes exist where we should not expect to find it.—Gold actually melted by the Heat which exists in the Air of the Atmosphere, where there is no Appearance of Fire, or of any Thing red-hot.—We ought to be cautious in attributing to the Action of unknown Powers, Effects similar to those produced by the Agency of Heat.—The most intense Heat may exist without leaving any visible Traces of its Existence behind it.—This important Fact illustrated by the necessary Result of an imaginary Experiment.

How far the possibility of the communication of Heat between the integrant particles of a Fluid

may or may not be owing to the extreme mobility of those particles, and to the infinitely short time that two of them, of different specific gravities, (owing to a difference of temperature) can remain in contact, I leave others to determine; in the mean time, it is most certain that the existence of this impossibility of any immediate communication of Heat among the particles of a Fluid renders the distribution of Heat very unequal; and it seems highly probable that many appearances which have been attributed to very different causes, are in fact owing to *intense Heat* existing and producing the effects proper to it in situations where its existence has not even been suspected.

If Fluids are non-conductors of Heat, no situation can possibly be more favourable to its preservation than when it exists in them; and it is not only evident *a priori* that the most intense Heat *may exist* in a few solitary particles of some Fluids, without its being possible for us to detect it, or to discover the fact, either by our feeling or by the thermometer; but there are many appearances that strongly indicate,—and others that prove, that intense Heat actually does exist in that concealed or imperceptible state very often.

There is no reason to suppose that it is possible for ice to be reduced to steam without being previously melted; and it is well known that ice cannot be melted with a lower degree of Heat than that of 32° of Fahrenheit's scale: but in the midst of winter, in the coldest climates, and when the temperature of air of the atmosphere, as shown by

the thermometer, has been much below 32° , ice, exposed to the air, has been found to evaporate.

How can we account for this event, except it be by supposing that some of the particles of air, which accidentally (as we express it) come into contact with the ice, are so hot, as not only to melt the small particles of ice which they happen to touch, but also to reduce a part of the generated water to steam, before it has time to freeze again; or by supposing that this is effected by intense Heat generated by light absorbed by small projecting points of the ice? As ice is a very bad conductor of Heat, that circumstance renders it more likely that the event in question should actually take place, in either of these ways.

If the metals were very bad conductors of Heat, instead of being very good conductors of it, I think it more than probable that even they would be found to evaporate, when exposed to the action of the direct rays of the sun; and perhaps also in situations in which such an event would appear still more extraordinary.

MERCURY has been actually found to *evaporate* under the mean temperature of the atmosphere!—What a striking proof is this that *fluid mercury* is a non-conductor of Heat;—and also, that very intense Heat may be generated, or exist, where it would not naturally be expected to be found. And does not the evaporation of water under the mean tem-

perature of the atmosphere afford another proof of this last fact?

That the most intense Heat is often excited in very small particles of solid bodies dispersed about in the midst of masses of cold liquids is not to be doubted. It is well known what an intense Heat the rays of the sun are capable of exciting; and it seems to be highly probable that Heat actually excited by them is always the same—that is to say—*intense in the extreme*: but when the rays are few, and when circumstances are not favourable to the *accumulation* of the Heat they generate, it is often so soon dispersed, that it escapes the cognizance of our senses, and of our instruments; and sometimes leaves no visible traces of its existence behind it.

Why should we not suppose that the Heat generated by a ray of light, which, entering a mass of cold water, accidentally meets with an infinitely small particle of any solid and opaque substance which happens to be floating in the liquid, and is absorbed by it, is not just as intense as that generated in the focus of the most powerful burning mirror, or lens?

Mr. Senebier has given us an account of a great number of interesting Experiments on the effects produced on different bodies by exposure to the direct rays of the sun; but why may we not attribute all those effects to the intense *local* Heat, generated by the light absorbed by the infinitely small—and, if I may use the expression—*insulated* particles
of

of the bodies which were found to be affected by it?

The surface of wood of various kinds was turned brown. The same appearances might be produced in a shorter time by the rays which proceed from a red-hot iron, which change the surface of the wood to an imperfect coal. But were not the surfaces of the woods which were turned brown by the light of the sun in Mr. Senebier's experiments changed to an imperfect coal?—And is it possible for a Heat less intense than that of *incandescence* to produce that effect?

Among the many facts that might be adduced to prove that the most intense Heat *may*, and frequently *does exist* where we should not expect to find it, the following appears to me to be very striking and convincing. It is, I believe, generally imagined that the intensity of the Heat generated in the combustion of fuel is much less in a small fire, than in a great one; but there is reason to think that this is an erroneous opinion, founded on appearances that are not conclusive; at least it is certain that the intense Heat of a large smelting furnace, such as is necessary for melting the most refractory metals, actually exists in the feeble flame of the smallest candle:—and what may appear still more extraordinary,—this intense degree of Heat often exists in the air of the atmosphere, *where no visible signs of Heat appear*, as I shall presently show.

Iron is fully *red-hot* by day light at the temperature of about 1000° of Fahrenheit's scale; brass

melts at 3807° ,—copper at 4587° ,—silver at 4717° ,—and gold at 5237° ; and nothing is more certain than that the Heat must be at that intensity which corresponds to the 5237th degree of Fahrenheit's scale, *where gold is found to melt*. But very fine gold, silver, or copper wire, flattened, (such as is used to cover thread to make lace) melts instantaneously on being held in the flame of a candle. It will even be melted if it be held a few seconds *over* the flame of a candle, *at the distance of more than an inch from the top of the flame*, in a place where there is no appearance of fire, or of any thing red-hot.

From the important information which we acquire from the result of these Experiments, we see how much we ought to be on our guard in forming an opinion with respect to the *intensity* of the Heat which *may exist* in the invisible insulated particles of matter of any kind that may be scattered about in a given space,—or which may float in any Fluid, where neither our feeling nor our thermometers can possibly be sensibly affected by it.

A thermometer can do no more than indicate the *mean of the different temperatures of all those bodies or particles of matter which happen to come into contact with it*. If it be suspended in air, it will indicate the mean of the temperatures of those particles of air *which happen to touch it*; but it can never give us any information respecting the *relative* temperatures of those particles of air.

If, during the most intense frost, a thermometer were suspended in the neighbourhood of a burning candle,—

candle,—in the same room for instance,—if it were placed over the candle, or nearly so, though it should be distant from it several feet, as air is a non-conductor of Heat, there is not the smallest doubt but that some solitary particles of air heated by the candle to the intense Heat of melting gold, would reach the thermometer; but neither the thermometer, nor the hand held in the same place, could give any indication of such an event.

As it appears from all that has been said that intense Heat *may exist* even under the form of *sensible Heat*, where its presence cannot be discovered or detected by us; and as it seems highly probable that in many cases, where its existence may escape our observation, it may nevertheless be capable of producing very visible effects, I think we ought always to be much on our guard in accounting for effects similar to those which are known to be produced by Heat; and never, without very sufficient reasons, attribute them to the agency of any other, *unknown* power: and this caution appears to me to be peculiarly necessary in accounting for those effects which have been found to be produced in various bodies when they are exposed to the action of the sun's rays.

If the solar rays concentrated in the focus of a lens, when they are made to fall on a piece of wood, instantly change its surface to a black colour, and reduce it to charcoal, why may we not conclude that the change of colour which is gradually or more slowly produced in the same kind of wood,
when

when it is simply exposed in the sun-beams, is produced in the same manner?

The difference in the *times* necessary to produce similar effects in these two cases is no proof that they are not produced *in the same manner*; for if they are effected merely by the agency of Heat, (which I suppose) then the effects produced in any given time will not be as the density of the light, or as the number of rays, but as that part of the Heat generated, which, not being immediately dispersed or carried off by the air, has time to produce the action proper to it in the wood; and consequently must be incomparably greater, in proportion, when the rays are concentrated, than when they are not.

Luna cornea exposed to the action of light changes colour;—but why should we not attribute this change to the expulsion of the oxygen united with the metal, by the agency of the Heat generated by the light? To remove every possible objection to this explanation of the phenomenon nothing more appears to be necessary than to admit, what is well known, that this metallic oxyd may be reduced, without addition, *with some degree of Heat*,—and that this substance is a bad conductor of Heat.

Will not the admission of our hypothesis respecting the *intensity* of the Heat which is supposed to be generated where light is stopped, and of that respecting the non-conducting power of Fluids with regard to Heat, enable us to account, in a manner

manner more satisfactory than has hitherto been done, for the effects of the sun's light in bleaching linen, when it is exposed wet to the action of his direct rays? as also for the reduction of those metallic oxyds which have been found to be revived by exposure to light?—And will it not also assist us in accounting for the production of pure air in the beautiful Experiment of Doctor Ingenhouz, in which the green leaves of living vegetables are exposed, immersed in water, to the sun's rays?

Mr. Senebier has shown that the colouring matter of healthy green leaves of vegetables, which is extracted from them by spirits of wine, and which tinges the spirits of a beautiful green colour, is destroyed, or rather changed to a dirty brown colour, in a few minutes, on exposing this tincture in a transparent phial, and *in contact with pure air*, to the direct rays of a bright sun:—but why should we not consider this process as a real combustion?

The Heat acquired by the liquid,—which, as I have often perceived in repeating the Experiment, is very considerable,—and the necessity there is for the presence of *pure air*, that the Experiment may succeed, seem to indicate that something very like combustion must take place in it.

If liquids are non-conductors of Heat, they ought certainly, *on that account*, to be peculiarly well calculated for confining, and consequently furthering the operations of that Heat which is generated by light, or by any other means, in their integrant particles, or in the infinitely small and insulated particles

ticles of other bodies that are dispersed about, or held in solution in them; as I have already more than once had occasion to observe.

If this supposition be admitted, a very great difficulty will be removed in accounting for chemical solution on the hypothesis that the change of form from a solid to a fluid state is in all cases a real fusion; or that it is effected by the sole agency of Heat;—and that concretion, or crystallization, is a process in all respects perfectly analogous to freezing.

There are but three forms under which sensible bodies are found to exist;—namely, that of a *solid*—that of a *fluid*—and that of an *elastic fluid*, or *gas*; and it is well known that every substance with which we are acquainted—all ponderable matter without exception,—is capable of existing alternately under all those forms indifferently; and that the form under which it appears *at any given time* depends on its *temperature at that time*.

We know farther that every identical substance undergoes these different changes of form at certain fixed temperatures: and when we consider the subject with attention we shall find that, had not these temperatures been fixed—and had they not been different in different bodies, it would have been utterly impossible for us to have identified any substance whatever.

Perhaps this is the only essential difference that really exists among bodies that appear to us to be different.

But

But not only the degrees of Heat, or points in the scale of temperature, at which the forms of different bodies are changed, are various; but the *extent of the variation of temperature* under which a substance can persevere, or continue to maintain its form in its *middle state*—that of *fluidity*—or rather *liquidity*,—is very different in different bodies: and this last circumstance has a wonderful effect in increasing the variety of the compositions and decompositions which are continually taking place in the various operations of nature on the surface of the globe.

Another circumstance, not less prolific in events, is the union which takes place between bodies of *different kinds*; and those most important changes in regard to the degrees of Heat which the bodies so united can support without having their forms changed, which are found to result from such union.

When, to the established laws which have been discovered in the operations of nature in the change of form in substances that appear to us to be *simple*, we add those which have been found to obtain in the changes of form of bodies that are known to be *compounded*, we shall perhaps be able to conceive some more distinct ideas with regard to the nature of those mechanical operations which take place in chemical processes.—I call them *mechanical*,—for mechanical they must of necessity be, according to the most rigid interpretation of that expression.

But the hypothesis of the existence of *intense Heat* in the midst of cold liquids is so new, and seems

seems to be so contrary to the result of all our experience and observation, that I feel it to be necessary to take some pains to illustrate the matter.

And first, we must not expect always to find traces remaining of the existence of intense Heat, even where there are the strongest reasons to think it has actually existed; for as often as Heat is dispersed, or carried off, before it has had time to produce any changes of form, or chemical changes or combinations in the bodies to which it is communicated, it leaves no marks behind it.

Fire-arms are often found to miss fire, even when many live sparks from the flint and steel actually fall into the pan among the priming; but nobody, surely, will pretend that the small particles of *red-hot iron* which fall among the grains of the gunpowder, and cool in contact with them, are not intensely hot;—incomparably more so than would be necessary to inflame the powder were their Heat of sufficient *duration* to produce that effect. Had these small sparks been invisible, it is highly probable that their existence would never have been suspected, and that the fact which they prove would not have been believed.

That gunpowder may be inflamed, it is necessary that the sulphur which constitutes one of its component parts should be first *melted*, and then *boiled*; for it is the vapour of boiling sulphur which always takes fire when gunpowder is kindled.

Were melted sulphur a conductor of Heat, there is reason to think that gunpowder would be very far from being so inflammable as we find it to be.

As

As those who have not been much accustomed to meditate on the subject under consideration may find some difficulty in conceiving how it is possible for intense Heat to be *excited* in, or to *exist* in the midst of a mass of any cold liquid, as of water for instance, without immediately producing visible effects, I feel it to be my duty to put that matter in the clearest light possible, and to show that what I have considered as being *probable* is most undoubtedly very far from being impossible.

The best method of proceeding in inquiries of this kind, where the principal object is to discover whether a supposed event, which, from its nature, cannot fall under the cognizance of our senses, is, or is not possible, seems to me to be, to begin by supposing the event to have actually taken place, and then to trace its necessary consequences, and compare them with those appearances which are actually found to take place.

Adopting this method, we will suppose a quantity of pure water, at the mean temperature of the atmosphere in England, that of 55° F., to be put into a clean and very transparent glass tumbler, placed in a window and exposed to the direct rays of the sun. If the glass and the water are both *perfectly transparent*, it is evident that no Heat will be generated in either of them by the sun's light.

If now a small particle of any opaque solid body be suspended in the midst of the water in the tumbler; those rays of light, which, impinging against it, are absorbed by it, must necessarily generate
Heat

Heat in the very moment when they are stopped. This is an incontrovertible fact, which nobody will dispute.

In order to render this imaginary Experiment more interesting, we will suppose the solid body put into the water to be a small particle of yellow amber; and that its specific gravity is so exactly equal to that of the water that it has no tendency to move in it, either upwards or downwards, and consequently will remain in the situation where it is placed, without being suspended; and we will suppose farther, that this solid particle of amber is nearly globular, and $\frac{1}{1000}$ of an inch in diameter, which is just equal to the diameter of a single thread of silk, as spun by the worm; and is probably one of the smallest objects that is perceptible by the human eye, unassisted by art.

As it is evident that Heat must be generated, or excited, in this small particle of amber, by the light it stops or absorbs, the points which remain to be discussed are, therefore, what *its intensity* is at the moment of its existence? and what are the effects which it ought to produce in consequence of that intensity?

The reasons have already been mentioned which render it probable that when Heat is generated by the rays of light its intensity, *where it is generated*—and before it has been diminished in consequence of its dispersion, is always the same: and taking it for granted that this is the case in fact, we will endeavour to trace the operations of that Heat,—
extreme

extreme in its intensity, or degree, but small in regard to its quantity, or to the space it occupies,—which is generated in the particle of amber in the Experiment under consideration.

As this Heat must first exist where it is generated, it is evident that it must exist at the surface of the particle of amber; and as all solid bodies are, in a greater or less degree, conductors of Heat, a part of this Heat will penetrate the substance of the solid particle, while another part of it will be carried off by the cold particles of water in contact with the surface thus heated by the light.

It remains therefore to be determined what the effects are which this Heat so absorbed, on the one hand, by the solid particle of amber, and communicated to the water on the other, ought necessarily to produce. And first, if the dispersion of the Heat by both these means should be sufficiently rapid to prevent its accumulation to such a degree as to melt the amber, it is evident that no visible effects by which its existence could be discovered would be produced in that substance; and this event—(the fusion of the amber)—will depend on three circumstances; namely, First, on the temperature at which amber melts;—Secondly, on the facility with which Heat expands and is dispersed in a solid mass of that substance, or on its conducting power;—and Thirdly, on the rapidity with which the Heat generated at the surface of the amber is carried off by the cold Fluid in which it is immersed.

Though I do not think there would be any reason for surprise, even admitting the existence of the supposed intense Heat, should the amber be found not to be melted under the circumstances described; yet, it appears to me to be extremely probable, that, if amber, in a very fine powder, were mixed with any transparent oil, capable of supporting a great degree of Heat without being reduced to vapour, and exposed in it to the direct rays of a very bright sun, the amber would melt, and be dissolved; though perhaps very slowly.

But if amber does not melt when exposed in water to the action of the sun's beams, and consequently suffers no visible change by which the existence of the Heat supposed to be generated at its surface by the light can be detected, ought not this Heat, were it in fact as intense as it is supposed to be, to produce some visible effects in the water, by which its existence would necessarily be discovered?

To resolve this doubt, we must inquire what visible effects it would be possible for the Heat in question to produce in the water. Now if we suppose the water not to be decomposed by this Heat, which, as no chemical change is supposed to take place in the amber, cannot happen, the only effect this Heat can possibly produce on the water is an increase of its temperature, which increase must, however, be much too small to be detected, either by the feeling, or by the thermometer.

It might perhaps be expected that *steam* would be found at the heated surface of the particle of
amber,

amber, and become visible; but when we consider the matter for a moment, we shall see that it is quite impossible that such an event should happen; for even on the supposition (which however is far from being probable)—that the same individual particles of water which come into contact with the hot surface of the amber should remain in contact with it till their temperatures should gradually be raised to that point at which water is changed to steam; yet, from the extreme rapidity with which steam condenses when in contact with cold water, it is evident that it could not exist an instant under the circumstances here supposed. Indeed we have direct proofs that steam cannot exist under such circumstances, by what is found to happen when large masses of iron, or steel, raised to a most intense heat, in a blast furnace, are suddenly plunged into cold water, by smiths, in tempering edge-tools; for these masses of red-hot metal may be distinctly seen to be in actual contact with the cold water; and did not a part of the water, which is decomposed by the hot iron, make its escape in the form of inflammable air, it is not probable that there would be any visible appearance from which the formation of steam could be suspected.

Hence we see the possibility of the existence of *intense Heat* in the midst of a mass of cold water, or of any other transparent liquid, without producing any visible effects; or leaving behind it any traces by which its existence could be suspected.

Let us now consider a case in which this intense Heat, though perfectly imperceptible on account

of the extreme minuteness of the particles of matter in which it exists, is capable nevertheless of producing very visible effects. Let us suppose a solution of nitro-muriate of gold, in water, to be exposed to the action of the sun's rays. If this solution were *perfectly* transparent, no Heat could possibly be generated in it by light; but as it is not so, Heat, in the highest degree of intensity, must necessarily be generated by those opaque particles (of the oxyd of gold) by which it is stopped. Now as gold is a very heavy substance, it is evident that it must be reduced to extremely small particles in order that, when changed to an oxyd by its union with oxygen, it may be dissolved in and continue suspended in water; and it is clear that the smaller any insulated particle of matter is, at the surface of which Heat is generated in consequence of the absorption of light, the more suddenly must the Heat so generated be dispersed through the whole substance of the particle, and the more equally and more intensely must that particle be heated: from hence it appears evidently, that, if the particles of the oxyd dispersed about in the water are but *small enough*, the Heat generated in them by the sun's rays will be sufficient to expel the oxygen united to the gold, and revive that metal.

There is one very obvious objection, that will doubtless be made to this conclusion, which, however, may easily be removed. The particle of the metallic oxyd which is supposed to be heated, is in contact with the water; how does it happen that
a great

a great part of this Heat does not immediately pass off into that cold Fluid? I might answer, because both water and steam are non-conductors of Heat;—and might adduce in support of this reason the well known fact, that a drop of water dropped on a piece of iron, heated to most intense white Heat, will remain some time on the iron without being evaporated, even considerably longer than if the iron were much less hot;—but a circumstance attending the beautiful Experiment in which iron is burned in oxygen gas, affords a more direct proof of the fact in question.

As this Experiment is commonly made, the iron, which is a piece of small wire, a few inches long, is introduced into a bottle, with a narrow neck, which contains the oxygen gas; the wire being fixed in its place, by causing its upper end to pass through a cork stopple, which is fitted to the mouth of the bottle. The lower end of the wire is pointed; and it is set on fire by being first heated in the flame of a candle, and then plunged suddenly, while red-hot, into the bottle. The combustion begins the moment the end of the wire enters the oxygen gas; and the metal continues to burn with the utmost violence, and with a copious emission of intense white light, till the wire, or till all the gas is consumed, affording one of the most brilliant and most interesting sights that can be imagined.

The product of this combustion is the oxygenation of the iron; and this metallic oxyd, in a state of fusion, and heated to the most intense white Heat,

falls to the bottom of the bottle in globules of different sizes.

To protect the glass against these drops of calx of iron in fusion, it is usual to leave a quantity of cold water in the bottle, enough, for instance, to cover its bottom to the height of about an inch : but I have frequently seen numbers of these globules, much smaller than peas, which have not only descended *red-hot* through the water ; but have remained red-hot at the bottom of the bottle, surrounded by the water, at least two or three seconds ; and actually melted the glass on which they reposed, (and as far as I can recollect,) without producing the smallest appearance of steam.

The water could not be decomposed, for the iron was already saturated with oxygen.

This Experiment will, I fancy, be considered as affording an indisputable proof that *intense Heat* may exist, at least for a short time, in a small particle of matter surrounded by a cold Fluid.

Now, as it has been found by actual Experiment, that when a solution of nitro-muriate of gold in water is exposed to the action of the sun's rays, the gold is revived ; and as it is known that an oxyd of gold may be reduced in the dry way, without addition, or merely by intense Heat, why should we not conclude that it is merely by *Heat* that that metal is revived in the case under consideration,—and that the *intensity* of the Heat by which this oxygenation is effected, is precisely the same in both cases ?

Should

Should this supposition be admitted, we might, perhaps, venture to proceed one step farther, and consider the nature and progress of the mechanical operations which take place in disoxygenation of metals, or their precipitation from a solution of their oxyds, when that operation is effected by means of Heat generated,—not by light,—but by the contact or union of infinitely small particles of bodies, different in kind, and disposed to generate or to absorb sensible Heat on coming together; which particles being dispersed about in the liquid solution, and in the substance added to it to effect the precipitation, are by this mixture brought into contact.

This would naturally lead us to an examination of the phenomena of solution,—and those clearly understood would, no doubt, give us a distinct view of the mechanical operations by which those tendencies to union are effected, which have been designated under the name *elective attraction*.

But how arduous an undertaking! what intense study!—what efforts of the imagination would be necessary to trace out and form distinct ideas of such a succession of events, all perfectly imperceptible by our organs, though assisted by all the resources of art!

Sensible of my own weakness, I dare not proceed any farther.—Perhaps it will be thought that I have already advanced much too far;—but it is right that I should acknowledge fairly, that in the present case, the temerity I have shown has not been entirely without design.

There are two ways in which philosophers, as well as other men, may be excited to action, and

induced to engage zealously in the investigation of any curious subject of inquiry:—they may be *enticed*,—and they may be *provoked*.

It will probably not escape the penetration of my reader, that I have endeavoured to use both these methods.—I am well aware of the danger that attends the latter of them; but the passionate fondness that I feel for the favourite objects of my pursuits frequently hurries me on far beyond the bounds which prudence would mark to circumscribe my adventurous excursions.

CHAP. IV.

An Account of a Variety of Miscellaneous Experiments.—Thermometers with cylindrical Bulbs may be used to show that Liquids are Non-conductors of Heat.—Ice-cold Water may be heated and made to boil standing on Ice.—Remarkable Appearances attending the thawing of Ice, and the melting of Tallow, and of Bees-Wax, by means of the radiant Heat projected downwards by a red-hot Bullet.—Beautiful Crystals of Sea-Salt formed in Brine standing on Mercury.—Olive Oil soon rendered colourless by Exposure to the Air standing on Brine.—An Attempt to cause radiant Heat from a red-hot Iron Bullet to descend in Oil.—Account of an artificial Atmosphere in which horizontal Currents were produced by Heat.—Conjectures respecting the proximate Causes of the Winds.

THOUGH this Essay is already grown to a much larger size than I originally intended, and even larger than I could have wished;—(well knowing how great an evil a great Book is generally thought to be)—I could not bring it to a conclusion without adding one Chapter more. In this Chapter the reader will find accounts of several Experiments, some of which he will probably consider as not altogether uninteresting. To take up as little of his time as possible I shall be very brief

in these accounts, and in general shall leave the reader to draw his own conclusions from the results of the Experiments I shall describe.

§ 1. *An Account of several simple Experiments, which show that Heat does not descend in Fluids.*

If a thermometer constructed with a long and narrow, naked cylindrical bulb,—(6 inches long, for instance, and $\frac{1}{2}$ an inch in diameter,)—and filled with mercury, oil, spirits of wine, or any other Fluid proper for that purpose, with which it is required to make the Experiment in question; such thermometer being at the temperature of the air in summer, or at any temperature above the point of freezing water, if the lower end, or half, of its bulb be plunged into a glass tumbler filled quite full to the brim with pounded ice and water, the height of the Fluid in the tube of the instrument will show that half the Fluid in the cylindrical bulb of the instrument is ice-cold, while the temperature of the other half of it remains unchanged.

The result will be the same, when, to prevent the communication of Heat from the air during the Experiment, that part of the bulb of the thermometer (the superior half of it) which projects above the level of the top of the tumbler is covered with a sheath lined with soft fur.

When more or less than half of the bulb of the thermometer is plunged into the ice and water, the height of the liquid in the tube of the instrument will

will show that that part only of the Fluid in the bulb is cooled which occupies the part of the bulb that is immersed in the ice and water.

§ 2. *Ice-cold Water, standing on Ice, may be heated and made to boil without melting the Ice, contrary to an Opinion that has generally prevailed,*

Take a thin glass tube, 1 inch in diameter, and about 8 or 10 inches long, containing about two or three inches of water, and by plunging the end of the tube into a freezing mixture of pounded ice and sea-salt cause the water in the tube to congeal: this being done, pour two or three inches of ice-cold water on the ice; and wrapping up about two inches of the lower end of the tube with a piece of flannel, and holding it inclined at an angle of about 45° ; by that part of it which is so covered, bring that part of the tube which is at the height of the surface of the Fluid-water to be just over the point of the flame of a burning candle, and distant from it about two or three inches. When the water in that part of the tube begins to boil, the tube may be advanced slowly over the flame of the candle; and if due care be taken to prevent a too sudden application of the Heat, all the water in the tube to within one quarter of an inch of the ice may be brought into the most violent ebullition before the ice will begin to be melted, and at last will appear to boil even at the very surface of the ice.

§ 3. *The*

§ 3. *The radiant Heat from a red-hot Iron Bullet does not appear to be able to make its Way downwards through liquid Water, nor through melted Tallow, nor melted Wax.*

1st Experiment.—A very small mercurial thermometer, with a naked globular bulb, was laid down in an horizontal position on two small projections of wax, in the bottom of a shallow wooden dish, in such a manner that the engraved scale of the thermometer lying uppermost, the height of the mercury in its tube could be observed. This being done, I poured cold water into the dish till it stood at the height of about $\frac{1}{4}$ of an inch above the bulb of the thermometer, and then presented to the thermometer an iron bullet about $1\frac{1}{2}$ inches in diameter, red-hot, which I held (by means of a fit handle) directly over its bulb at the distance of about an inch.

The thermometer seemed to take very little notice of the vicinity of the red-hot iron.

When its bulb was covered with oil the result of the Experiment was much the same; but when it was exposed naked, or uncovered by a liquid, to the rays from the hot iron, it appeared to acquire Heat very rapidly. But the two following Experiments were still more decisive and satisfactory.

2^d Experiment.—A shallow earthen dish, about 3 inches deep and 12 inches in diameter at its brim, was filled with water, and being exposed in a cold room in winter, the water was frozen, and formed a cake

a cake of ice at its surface, about an inch thick. Letting the dish remain in its place, in order that the surface of the ice might remain perfectly horizontal, (which was necessary to the complete success of the Experiment, as will presently be seen) I entered the room with a chafing-dish filled with live coals, in the midst of which was my iron bullet, perfectly red-hot; and taking out the bullet from among those burning coals, I held it over the centre of this horizontal sheet of ice, and distant from it about $\frac{1}{8}$ of an inch.

The ice directly under the red-hot bullet was soon thawed; but the depth to which it was thawed was very inconsiderable: the water, however, extended itself slowly from the centre towards the circumference, and at length a circular spot 2 or 3 inches in diameter in the centre of the surface of the ice was covered with it, though but to a very inconsiderable depth.

This little spreading sea appeared to prey on the wall of ice by which it was surrounded on every side.

The particles of water in contact with this wall, being rendered specifically lighter on becoming ice-cold, they move upwards, and making way for other warmer particles to advance from below, cause currents in opposite directions to set between the centre (where the hot-iron remains) and the circumference.—As a current at the temperature of 41° must necessarily set downwards at the middle of the circle, this current striking against the middle of the excavation formed in the ice ought to

to deepen it gradually in that part, though but slowly,—and this is what was actually found to be the case; for the bottom of this excavation was not perfectly flat, but was deeper at and near its centre than at its sides.

3d Experiment.—When this Experiment was varied by using a flat cake of tallow instead of a cake of ice, a very extraordinary appearance indeed presented itself, which at first surprised me very much, but which I soon perceived was a new, and very striking proof that Fluids are non-conductors of Heat.

The bottom of the circular cavity in the cake of tallow which was occupied by that part of the tallow that had been melted in the Experiment, instead of being concave, as I had found that in the ice to be,—or flat,—as I expected to find this,—was *convex* in the middle, or rather rose up in the form of a protuberance, or very blunt point, the extremity of which reached almost to the surface of the melted tallow! As the iron bullet was held as near as possible to the tallow, the end of this projection, which remained unmelted, was certainly not more than $\frac{2}{10}$ of an inch distant from this red-hot ball! Reflecting on the unexpected result of this Experiment I was much struck, and not a little humiliated, with the proof it seemed to me to afford of the impossibility of predicting with certainty any event, however inevitable it may appear, which has not actually been seen to happen.

Though

Though I well knew how the Heat must be communicated under the given circumstances, and could foretell with certainty the directions of the currents it must necessarily occasion in the melting tallow; yet, the utmost efforts of my intellectual powers, exercised as they were by much meditation, were not sufficient to enable me to foresee that the point where least Heat would be communicated was that precisely which was nearest to the red-hot bullet; and that a protuberance of unmelted tallow would be left in that place.

Let those be very cautious who speculate on the supposed results of Experiments they have never made!

On repeating this Experiment, and varying it by using a cake of fine bleached *bees-wax*, instead of tallow, the result was much the same: the protuberance, however, in the middle of the circular cavity occupied by the melted wax, though perfectly perceptible, was less considerable, in height, than that in the cake of tallow.

§ 4. *Beautiful Crystals of Sea-Salt formed in Brine standing on Mercury.*

A small quantity of strong brine, standing on mercury in an open glass tumbler, having by accident been left in a room in a retired part of the house, I observed at the end of about six months, that two beautiful crystals of salt, perfectly quadrangular, had been formed in it, one of which was $\frac{1}{4}\frac{1}{6}$ of an inch long, $\frac{1}{4}\frac{1}{6}$ of an inch wide, and

and $\frac{5}{8}$ of an inch in thickness; and the other $\frac{13}{16}$ of an inch long, $\frac{10}{16}$ of an inch wide, and $\frac{13}{16}$ of an inch thick.

Did the Fluid mercury on which this brine reposed contribute?—and how?—to the regularity of the form, and the uncommon size of these crystals?—And might not beautiful crystals of other salts be procured by similar means?

§ 5. *Olive Oil rendered colourless by Exposure to the Air standing on Brine.*

A quantity of *olive oil*, about $\frac{3}{4}$ of an inch in depth, having by accident been left standing in an open glass jar, about four inches in diameter, on about a quart of brine, moderately strong, in a retired room, where the sun's rays never enter; at the end of about six months I observed that the oil had become perfectly colourless, and appeared to me to be nearly as transparent as the purest water. On the approach of winter I found that this oil was much more liable to be congealed with cold than oil of the same kind which had stood near it many months in a large glass bottle closed with a cork.

§ 6. *An unsuccessful Attempt to cause radiant Heat from a red-hot Iron Bullet to descend in Oil.*

Having poured a quantity of this colourless oil into a glass tumbler, and caused it to congeal throughout, I presented to its upper surface a red-hot iron bullet, $1\frac{1}{2}$ inches in diameter, and held it

it quite close to the oil, several minutes, till the bullet ceased to be red-hot. As the oil seemed rather to be merely thickened by the cold, and to have lost its transparency in consequence of the presence of a number of opaque particles, which were everywhere dispersed about in it, than to be congealed into a solid mass, I thought that if it were possible for radiant Heat to descend in any Fluid it might perhaps be in this; and if this should happen, I was certain to make the discovery by the manner in which the oil recovered its transparency; for should radiant Heat descend, the form of the mass of oil first restored to its transparency must necessarily have been *hemispherical*, or some section of a sphere, or at least of some convex figure: but the under part of that part of the oil which was restored to its transparency in this Experiment was, to all appearance, as perfectly flat and horizontal as the upper surface of it, which proves that the Heat, by which the congealed oil was thawed, was communicated to it,—not immediately by the red-hot bullet,—but *mediately* by means of the Heat absorbed by or generated in the sides of the tumbler. This Experiment appears to me to be important in many respects; but it would be foreign to my present purpose to engage in an investigation of the subject with which it is most intimately connected.

I cannot finish this Essay without giving my reader an account of one more Experiment, the result of which was not only quite unexpected, but uncommonly interesting.

Happening accidentally to place in a window the little instrument I had contrived for rendering visible the internal motions which are occasioned in water when Heat is propagated in that Fluid *, as it was winter, and the room was warmed by a German stove, that side of the instrument which happened to be nearest the window being exposed to a current of cold air, while the instrument received Heat continually on the other side from the warmer air of the room, the liquid in the instrument was thrown into motions which never ceased, and afforded a very interesting sight.

With a view merely to amuse myself, and the friends who should happen to call in to visit me,—and without the smallest expectation of making any new discoveries,—I contrived, and caused to be executed, the instrument I am now about to describe, which I thought could not fail to render these motions perpetual, and exhibit them in a striking manner.

A flat box was formed of two equal panes, each 13 inches high, and $10\frac{1}{2}$ inches wide, of fine ground glass, fitted into a square frame of brass in such a manner, that these two panes (which are parallel to each other) are at the distance of 1 inch from each other. In the middle of the top of this brass frame there is a circular opening about $\frac{1}{2}$ an inch in diameter, into which a projecting cylindrical brass tube, about half an inch in length, is soldered; and in the middle of the bot-

* For a description of this instrument see Chapter II. of this Essay.

tom of the frame there is a similar tube which projects downward. The first of these openings serves for introducing into the flat box the liquid with which it is filled; and the other for drawing it off; and they are both well closed with fit stopples of cork.

On both sides of this brass frame there are deep grooves into which the panes of glass are fitted, and the box was made water-tight by luting the joinings of the glass with the frame with glaziers putty. On the outside of the frame there are thin projections of sheet brass, by means of which the box was fixed in one of the sashes of a window in my room, where it occupied the place of a pane of glass, which was removed to make way for it. This window fronts the south-east, and consequently is exposed to the sun a great part of the day.

Having provided a sufficient quantity of the saline solution,—(of the same kind as was used in constructing the instrument above mentioned, contrived for rendering visible the internal motions in Fluids) and having mixed with it a due proportion of pulverized yellow amber, I now filled the box half full with this mixture; and as the air in the room was considerably warmer than that without, I expected that the motions in the liquid occasioned by the passage of the Heat would immediately commence.

This actually happened;—but how great was my surprise, when, instead of the vertical currents I expected, I discovered horizontal currents,

running in opposite directions,—one above another,—or regular WINDS,—which, springing up in the different regions of this artificial atmosphere, prevailed for a long time with the utmost regularity ;—while the small particles of the amber collecting themselves together formed clouds of the most fantastic forms, which being carried by the winds, rendered the scene perfectly fascinating !

It would be impossible to describe the avidity with which I gazed on these enchanting appearances.

In the state of enthusiasm I then was, it really seemed to me that Nature had for a moment drawn back the veil with which she hides from mortal eyes her most secret and most interesting operations ;—and that I now saw the machinery at work by which winds and storms are raised in the atmosphere !

Nothing seemed to be wanting to complete this bewitching scene, and give it the air of perfect enchantment, but that lightning, in miniature, should burst from these little clouds : and they were frequently so thickened up, and had so much the appearance of preparing for a storm, that had that event actually taken place, it could hardly have increased my wonder and ecstasy.

There were several accidental circumstances attending this Experiment, which contributed to render it more interesting. The sun, which happened to be remarkably bright, shone full upon the window where the apparatus was placed ; and as the grooves in the frame in which the plates of glass were

were fixed were not deep, that part of this frame which formed the narrow bottom of the box being exposed to the sun's rays, a considerable quantity of Heat was generated by them in that place, as appeared by the motions of the particles of pulverized amber which lay on the bottom of the box, or those which were brought there by the currents.

When these particles, on being heated by the sun-beams, began to move, they first arose up nearly perpendicularly; but before they had risen to any considerable height, they were carried away obliquely, and nearly in an horizontal direction, by the lower current, answering to the wind which, in the atmosphere, prevails at the surface of the earth.

The perpendicular rise of these particles from the bottom of the box, and the subsequent change of their direction, called to my remembrance an appearance very common in hot countries, which I recollected to have often seen, and by which I had often been amused in my youth: in very hot and dry weather, when the wind is still and the sun very powerful, the air which lies on the ground often appears in the most violent agitation, resembling that of a boiling liquid; which motion is most rapid at the surface of the earth, and appears to cease at the height of five or six feet above the ground.

Is not this violent agitation occasioned by the conflict which takes place between the hot and the comparatively cold air moving *vertically*, and in

opposite directions, very near the surface of the ground? And are not the winds which prevail above, occasioned by the efforts of whole *strata* of air to ascend or descend obliquely?

The currents I observed to prevail in my artificial atmosphere were never perfectly horizontal; and if my suspicions with respect to the cause of the winds are well founded, neither can those winds be horizontal which prevail in the superior regions of the atmosphere of the earth, though they may be very nearly so.

The greatest velocity of the currents in the saline liquid in this Experiment was nearly two inches in a minute, but their motions were in general much slower. As the windows in the room in which this Experiment was made are double, (as are all those both in summer and winter in the apartment I inhabit) and as the apparatus above described occupied the place of a pane of glass belonging to the inside-window, it was in my power, by opening either the inside-window or the outside-window, to cause the Heat on the two opposite sides of the box to be either equal or unequal at pleasure; and by variations which that arrangement enabled me to make in the Experiments I produced several interesting appearances.

There was one very striking appearance indeed, which never failed to present itself regularly every day during the three weeks that the Experiment was continued *. The clouds, after having been

* An end was put to the Experiment by an accident; the box being broken by the carelessness of a servant in shutting the window-shutter.

driven about all day by the different currents in the liquid, (of which there were sometimes as many as six or seven, running in opposite directions at the same time) never failed to collect themselves together in the evening, into large masses; sometimes forming only one, and sometimes two or three *strata* at different heights, where they remained, to all appearance perfectly motionless, during the night.

There can be no question with respect to the *proximate* cause of this phenomenon; for it was undoubtedly owing to a diminution or total cessation of the operation of that cause,—of those causes,—or of some of them,—by which an inequality of temperature in the liquid was produced and continued;—but it would be highly curious to investigate the more remote causes of this appearance, and see how far *light*, or rather the absence of it, was concerned in producing it: but that discussion would lead me into a very abstruse inquiry,—that respecting radiant Heat,—which would take up more time than I am at present able to bestow on it. Perhaps I may find leisure and courage at some future period to attempt that most difficult investigation. My reader will doubtless have observed that I have hitherto taken pains to avoid it.

I cannot take my leave of the Experiment I have been describing without giving my reader a faithful account of every thing I can recollect respecting it; and particularly of one accidental circumstance, which, it is possible, may have had some share in
producing

producing the interesting appearances which so powerfully attracted my attention.

The saline liquor and the pulverized amber were mixed in a bottle, and were not put into the flat box till after it had been fixed in the sash or frame of the window; but when I came to pour this mixture into the box I found that I had not provided enough of it. To supply this defect, without the trouble of emptying the box, I added, at several different times, pure water, and a strong solution of pot-ash, in such proportions as I knew to be proper to produce the specific gravity required; and then endeavoured to mix the whole as intimately as possible by agitating the liquor for some considerable time by means of a long and strong quill, the end of which I thrust down into the box through the hole by which the liquor was introduced.

Whether those different portions of liquid were in fact intimately mingled by these means, I cannot positively determine. They certainly had every appearance of being so; for the amber was evidently well mixed, and very equally distributed in every part of the Fluid. But even should we grant that the liquid remained divided in different *strata*, arranged according to the specific gravities of the different portions of it that were poured into the box at different times, it does not appear to me that the result of the Experiment would be less interesting on that account, or the application of it less satisfactory in explaining the cause of the winds in the atmosphere.

I am, however, far from being desirous that much stress should be laid on this single Experiment, being perfectly sensible that others may be contrived, the results of which would be more decisive: in the mean time it appears to me that the hint given us is too plain not to deserve some attention. If it should awaken the curiosity of experimental philosophers, and excite them to farther investigation, the end I had principally in view in publishing this account of it will be completely answered.

DESCRIP.

I am, however, the first to say that much
has been done, and that the people of the
country are now in a position to be
satisfied with the results of the
work which has been done. It is
true that the work has not been
done in a perfect manner, but it
has been done in a manner which
has been satisfactory to the
people of the country. It is true
that the work has not been done
in a perfect manner, but it has
been done in a manner which
has been satisfactory to the
people of the country.

Fig. 4.

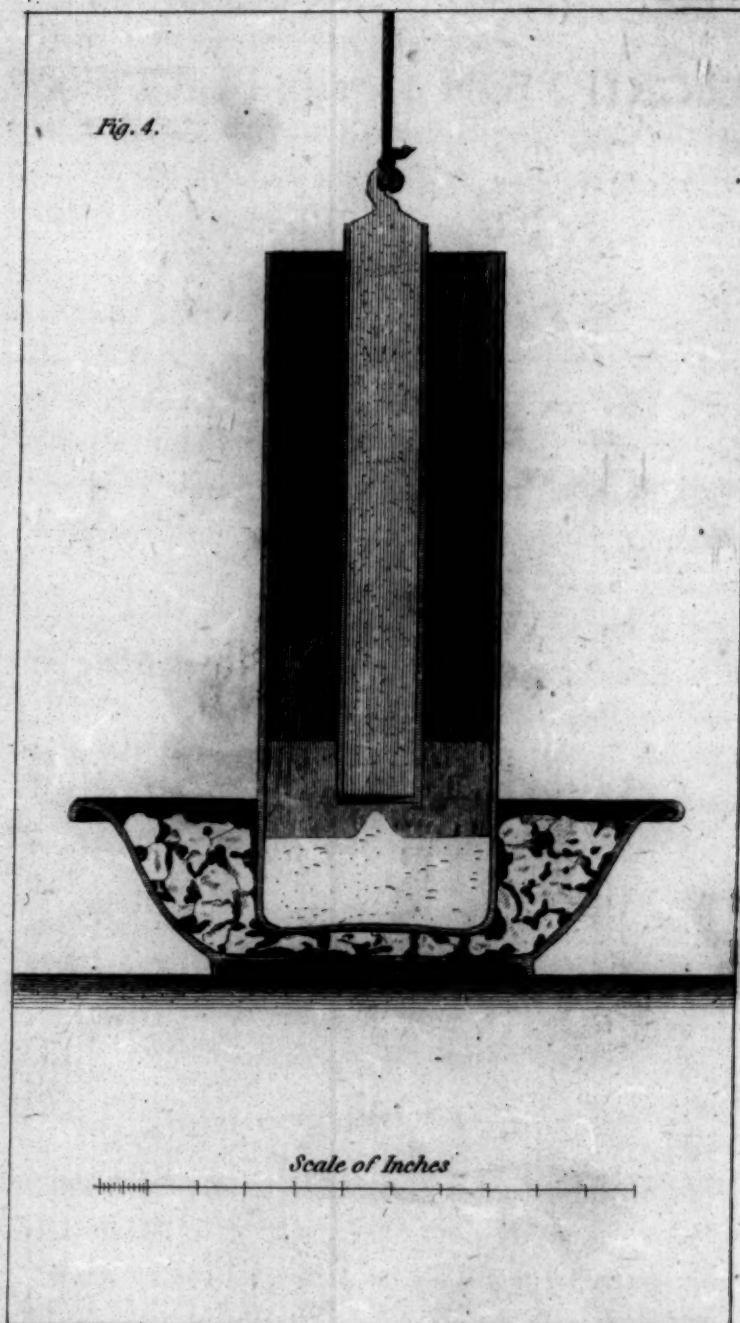
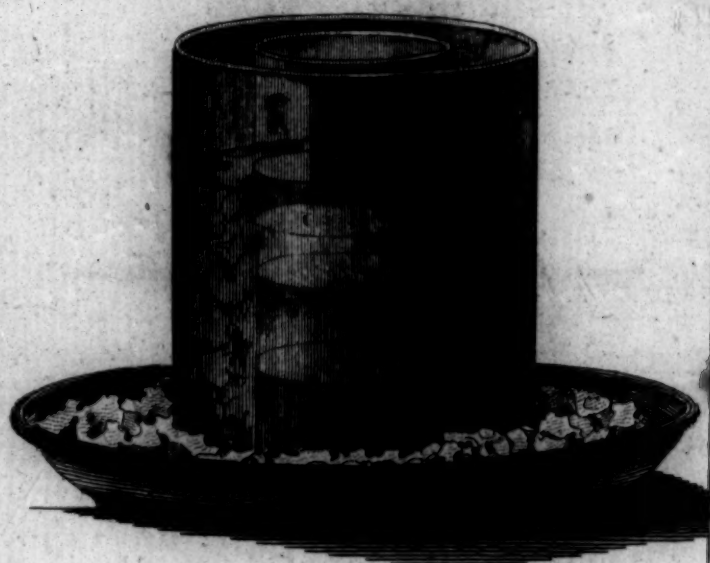


Fig. 5.



Scale of Inches

Neale's engraving

DESCRIPTION OF THE PLATES.

P L A T E I I I .

FIG. 4. This Figure represents a vertical section of the apparatus used in the Experiment No. 55. (see page 315), in which an attempt was made to melt the top of a projecting point of ice by Heat transmitted *downwards* through olive-oil communicated by a solid cylinder of iron, heated in boiling water.

In this Figure the tall glass jar (in the bottom of which the cake of ice was frozen) is standing in an earthen pan filled with pounded ice.

The *oil* is also represented standing on the *cake of ice* in the jar; and the iron cylinder in its sheath of paper suspended in the axis of the jar in such a manner that the lower end of this cylinder, which is flat, is directly over the pointed projection of ice, and distant from it $\frac{1}{16}$ of an inch.

P L A T E I V .

Fig. 5. This Figure shows the manner in which the Experiment No. 57. (see page 326) was made, when pure or fresh water in a glass jar was made
to

to repose on brine, or water saturated with sea-salt, without mixing with it.

In this Experiment the smaller jar, which contained the brine, the pure water, and a quantity of olive-oil by which the surface of the pure water was covered, stood in a larger glass jar,—which last stood in a shallow earthen dish filled with pounded ice and water.

The space between the outside of the smaller jar and the inside of the larger jar was filled, to the height of about an inch above the level of the surface of the oil in the smaller jar, with pieces of ice nearly as large as walnuts, and ice-cold water.

END OF PART II. OF ESSAY VII.

E S S A Y VIII.

OF THE
PROPAGATION OF HEAT
IN
VARIOUS SUBSTANCES:

BEING

An Account of a Number of NEW EXPERIMENTS made
with a View to the Investigation of the CAUSES of the
WARMTH of NATURAL and ARTIFICIAL CLOTHING.

First published in the Philosophical Transactions,

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OF THE
EIGHTH ESSAY.

CHAP. I.

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ESSAY IX.

**AN INQUIRY concerning the SOURCE of the HEAT
which is EXCITED by FRICTION.** Page 467

INTRODUCTION.

THIS Essay contains nothing that will be new to philosophical readers; for it is little more than the substance of two Papers which have already appeared in the Philosophical Transactions of the Royal Society of London; one in the year 1786; and the other (for which the Author had the honour to receive from the Society the Copleian Annual Medal) in the year 1792.

As reference has frequently been made to these Papers in several of the preceding Essays; and as many of the Experiments of which an account is given in them are not only interesting in themselves, but are necessary to be known in all their details in order to judge of several important conclusions that have been founded on their results, the Author has thought that it would not be improper to republish them under the present form. He was also desirous of adding the substance of those Papers to his Sixth and Seventh Essays, in order that all that he has written on the *Science of Heat* might be brought together in one volume.

The Essays which are destined to compose the next volume (many of which are already in great forwardness) are all on practical subjects of a popular nature, and of general utility; and on that

account it was judged best to keep them separate from those contained in this volume, which partake more of the nature of abstruse philosophical investigations.

Various unforeseen events have contributed to retard the publication of the promised Essays on Kitchen Fire-places—on Cottage Fire-places—and on Clothing; but the Author has well-founded hopes of being able to bring them forward in the course of a few months.

E S S A Y VIII.

Of the PROPAGATION of HEAT in various Substances.

CHAP. I.

An Account of the Instruments that were prepared for making the proposed Experiments.—A Thermometer is constructed whose Bulb is surrounded by a TORRICELLIAN VACUUM.—Heat is found to pass in a Torricellian Vacuum with greater Difficulty than in Air.—Relative conducting Powers of a Torricellian Vacuum and of Air with regard to Heat determined by Experiment.—Relative conducting Powers of dry Air and of moist Air.—Relative conducting Powers of Air of different Degrees of Density.—Relative conducting Powers of MERCURY; WATER; AIR; and a TORRICELLIAN VACUUM.

[Read before the ROYAL SOCIETY, March 9, 1786.]

EXAMINING the conducting power of air, and of various other fluid and solid bodies, with regard to Heat, I was led to examine the conducting power of the *Torricellian vacuum*. From the striking analogy between the electric fluid and Heat respecting their conductors and non-conductors,

(having found that bodies, in general, which are conductors of the electric fluid, are likewise good conductors of Heat, and, on the contrary, that electric bodies, or such as are bad conductors of the electric fluid, are likewise bad conductors of Heat,) I was led to imagine that the Torricellian vacuum, which is known to afford so ready a passage to the electric fluid, would also have afforded a ready passage to Heat.

The common experiments of heating and cooling bodies under the receiver of an air-pump I conceived to be inadequate to determining this question; not only on account of the impossibility of making a perfect void of air by means of the pump; but also on account of the moist vapour, which exhaling from the wet leather and the oil used in the machine, expands under the receiver, and fills it with a watery fluid, which, though extremely rare, is yet capable of conducting a great deal of Heat: I had recourse therefore to other contrivances.

I took a thermometer, unfilled, the diameter of whose bulb (which was globular) was just half an inch, Paris measure, and fixed it in the centre of a hollow glass ball of the diameter of $1\frac{3}{4}$ Paris inch, in such a manner, that the short neck or opening of the ball being soldered fast to the tube of the thermometer $7\frac{1}{2}$ lines above its bulb, the bulb of the thermometer remained fixed in the centre of the ball, and consequently was cut off from all communication with the external air. In the bottom of the glass ball was fixed a small hollow tube or point, which projecting outwards was soldered

to the end of a common barometer tube about 32 inches in length, and by means of this opening the space between the internal surface of the glass ball and the bulb of the thermometer was filled with hot mercury, which had been previously freed of air and moisture by boiling. The ball, and also the barometrical tube attached to it, being filled with mercury, the tube was carefully inverted, and its open end placed in a bowl in which there was a quantity of mercury. The instrument now became a barometer, and the mercury descending from the ball (which was now uppermost) left the space surrounding the bulb of the thermometer free of air. The mercury having totally quitted the glass ball, and having sunk in the tube to the height of 28 inches, (being the height of the mercury in the common barometer at that time) with a lamp and a blow-pipe I melted the tube together, or sealed it hermetically, about three-quarters of an inch below the ball, and cutting it at this place with a fine file, I separated the ball from the long barometrical tube. The thermometer being afterwards filled with mercury in the common way, I now possessed a thermometer whose bulb was confined in the centre of a *Torricellian vacuum*, and which served at the same time as the body to be heated, and as the instrument for measuring the Heat communicated.

Experiment, N° 1.

With this instrument (see Fig. 1.) I made the following Experiment. Having plunged it into a

vessel filled with water, warm to the 18th degree of REAUMUR's scale, and suffered it to remain there till it had acquired the temperature of the water, that is to say, till the mercury in the inclosed thermometer stood at 18° , I took it out of this vessel and plunged it suddenly into a vessel of boiling water, and holding it in the water (which was kept constantly boiling) by the end of the tube, in such a manner that the glass ball, in the centre of which was the bulb of the thermometer, was just submerged, I observed the number of degrees to which the mercury in the thermometer had arisen at different periods of time, counted from the moment of its immersion. Thus, after it had remained in the boiling water 1 min. 30 sec. I found the mercury had risen from 18° to 27° . After 4 minutes had elapsed, it had risen to $44^{\circ}\frac{9}{18}$; and at the end of 5 minutes it had risen to $48^{\circ}\frac{2}{18}$.

Experiment, N^o 2.

Taking it now out of the boiling water I suffered it to cool gradually in the air, and after it had acquired the temperature of the atmosphere, which was that of 15° R. (the weather being perfectly fine) I broke off a little piece from the point of the small tube which remained at the bottom of the glass ball, where it had been hermetically sealed, and of course the atmospheric air rushed immediately into the ball. The ball surrounding the bulb of the thermometer being now filled with air, (instead of being emptied of air, as it was in the before-mentioned Experiment,) I resealed the end
of

of the small tube at the bottom of the glass ball hermetically, and by that means cut off all communication between the air confined in the ball and the external air; and with the instrument so prepared I repeated the Experiment before-mentioned; that is to say, I put it into water warmed to 18° , and when it had acquired the temperature of the water, I plunged it into boiling water, and observed the times of the ascent of the mercury in the thermometer. They were as follows:

	Time elapsed.	Heat acquired.
Heat at the moment of being plunged into the boiling water, - - - - }		18° R.
After having remained in the boiling water	M. S.	°
	0 45	27
	1 0	$34\frac{1}{3}$
	2 10	$44\frac{2}{3}$
	2 40	$48\frac{2}{3}$
	4 0	$56\frac{2}{3}$
	5 0	$60\frac{2}{3}$

From the result of these Experiments it appears evidently, that the Torricellian vacuum, which affords so ready a passage to the electric fluid, so far from being a good conductor of Heat, is a much worse conductor of it than common air, which of itself is reckoned among the worst: for in the last Experiment, when the bulb of the thermometer was surrounded with air, and the instrument was plunged into boiling water, the mercury rose from 18° to 27° in 45 seconds; but in the former Experiment, when it was surrounded by a Torricellian vacuum, it required to remain in the boiling water 1 minute

minute 30 seconds = 90 seconds, to acquire that degree of heat. In the vacuum it required 5 minutes to rise to $48^{\circ}\frac{2}{10}$; but in air it rose to that height in 2 minutes 40 seconds; and the proportion of the times in the other observations is nearly the same, as will appear by the following Table.

The bulb of the thermometer placed in the centre of the glass ball, and			
surrounded by a Torricellian vacuum. (Exp. N ^o 1.)		surrounded by air. (Exp. N ^o 2.)	
Time elapsed.	Heat acquired.	Time elapsed.	Heat acquired.
Upon being plunged into } boiling water - }		18°	
M. S.	°	M. S.	°
After remaining in it	1 30 27	0 45 27	
—	—	1 0 30 $\frac{4}{10}$	
4 0 44 $\frac{2}{10}$		2 10 44 $\frac{2}{10}$	
5 0 48 $\frac{2}{10}$		2 40 48 $\frac{2}{10}$	
—	—	4 0 56 $\frac{2}{10}$	
—	—	5 0 60 $\frac{2}{10}$	

These Experiments were made at Manheim, upon the first day of July 1785, in the presence of Professor Hemmer, of the Electoral Academy of Sciences of Manheim, and Charles Artaria, meteorological instrument maker to the academy, by whom I was assisted in making them.

Finding the construction of the instrument made use of in these Experiments attended with much trouble and risk, on account of the difficulty of foldering the glass ball to the tube of the thermometer without at the same time either closing up, or otherwise injuring, the bore of the tube, I had

had recourse to another contrivance much more commodious, and much easier in the execution.

At the end of a glass tube or cylinder about eleven inches in length, and near three quarters of an inch in diameter internally, I caused a hollow globe to be blown $1\frac{1}{4}$ inch in diameter, with an opening in the bottom of it corresponding with the bore of the tube, and equal to it in diameter, leaving to the opening a neck or short tube, about an inch in length. Having a thermometer prepared, whose bulb was just half an inch in diameter, and whose freezing point fell at about $2\frac{3}{4}$ inches above its bulb, I graduated its tube according to Reaumur's scale, beginning at 0° , and marking that point, and also every tenth degree above it to 80° , with threads of fine silk bound round it, which being moistened with lac varnish adhered firmly to the tube. This thermometer I introduced into the glass cylinder and globe just described, by the opening in the bottom of the globe, having first choaked the cylinder at about 2 inches from its junction with the globe by heating it, and crowding its sides inwards towards its axis, leaving only an opening sufficient to admit the tube of the thermometer. The thermometer being introduced into the cylinder in such a manner that the centre of its bulb coincided with the centre of the globe, I marked a place in the cylinder, about three-quarters of an inch above the 80th degree or boiling point upon the tube of the inclosed thermometer, and taking out the thermometer, I choaked the cylinder again in this place.

Intro-

Introducing now the thermometer for the last time, I closed the opening at the bottom of the globe at the lamp, taking care before I brought it to the fire, to turn the cylinder upside down, and to let the bulb of the thermometer fall into the cylinder till it rested upon the lower choak in the cylinder. By this means the bulb of the thermometer was removed more than 3 inches from the flame of the lamp. The opening at the bottom of the globe being now closed, and the bulb of the thermometer being suffered to return into the globe, the end of the cylinder was cut off to within about half an inch of the upper choak. This being done, it is plain, that the tube of the thermometer projected beyond the end of the cylinder. Taking hold of the end of the tube, I placed the bulb of the thermometer as nearly as possible in the centre of the globe, and observing and marking a point in the tube immediately above the upper choak of the cylinder, I turned the cylinder upside down, and suffering the bulb of the thermometer to enter the cylinder, and rest upon the first or lower choak, (by which means the end of the tube of the thermometer came further out of the cylinder) the end of the tube was cut off at the mark just mentioned, (care having first been taken to melt the internal cavity or bore of the tube together at that place) and a small solid ball of glass, a little larger than the internal diameter or opening of the choak, was soldered to the end of the tube, forming a little button or knob, which resting upon the upper choak of the cylinder served to suspend the thermometer

meter in such a manner that the centre of its bulb coincided with the centre of the globe in which it was shut up. The end of the cylinder above the upper choak being now heated and drawn out to a point, or rather being formed into the figure of the frustum of a hollow cone, the end of it was foldered to the end of a barometrical tube, by the help of which the cavity of the cylinder and globe containing the thermometer was completely voided of air with mercury; when, the end of the cylinder being hermetically sealed, the barometrical tube was detached from it with a file, and the thermometer was left completely shut up in a Torricellian vacuum, the centre of the bulb of the thermometer being confined in the centre of the glass globe, without touching it in any part, by means of the two choaks in the cylinder, and the button upon the end of the tube. (See Fig. 2.)

Of these instruments I provided myself with two, as nearly as possible of the same dimensions; the one, which I shall call N^o 1. being voided of air, in the manner above described; the other, N^o 2. being filled with air, and hermetically sealed.

With these two instruments (see Fig. 2.) I made the following Experiments upon the 11th of July last at Manheim, between the hours of ten and twelve, the weather being very fine and clear, the mercury in the barometer standing at 27 inches 11 lines, Reaumur's thermometer at 15°, and the quill hygrometer of the academy of Manheim at 47°.

Experiments,

Experiments, N° 3, 4, 5, and 6.

Putting both the instruments into a mixture of pounded ice and water, I let them remain there till the mercury in the inclosed thermometers rested at the point 0° , that is to say, till they had acquired exactly the temperature of the cold mixture; and then taking them out of it I plunged them suddenly into a large vessel of boiling water, and observed the time required for the mercury to rise in the thermometers from ten degrees to ten degrees, from 0° to 80° , taking care to keep the water constantly boiling during the whole of this time, and taking care also to keep the instruments immersed to the same depth, that is to say, just so deep that the point 0° of the inclosed thermometer was even with the surface of the water.

These Experiments I repeated twice with the utmost care; and the following Table gives the result of them.

Thermometer N ^o 1.			Thermometer N ^o 2.		
Its bulb half an inch in diameter, shut up in the centre of a hollow glass globe, $1\frac{1}{2}$ inch in diameter, void of air, and hermetically sealed.			Its bulb half an inch in diameter, shut up in the centre of a hollow glass globe, $1\frac{1}{2}$ inch in diameter, filled with air, and hermetically sealed.		
Taken out of freezing water, and plunged into boiling water.			Taken out of freezing water, and plunged into boiling water.		
Time elapsed.		Heat acquired.	Time elapsed.		Heat acquired.
Exp. N ^o 3.	Exp. N ^o 4.		Exp. N ^o 5.	Exp. N ^o 6.	
M. S.	M. S.	0°	M. S.	M. S.	0°
0 51	0 51	10	0 30	0 30	10
0 59	0 59	20	0 35	0 37	20
1 1	1 2	30	0 41	0 41	30
1 18	1 22	40	0 49	0 53	40
1 24	1 23	50	1 1	0 59	50
2 0	1 51	60	1 24	1 20	60
3 30	3 6	70	2 45	2 25	70
11 41	10 27	80	9 10	9 38	80
22 44	21 1	= total time	16 55	17 3	= total time
of heating from 0° to 80°.			of heating from 0° to 80°.		
Total time from 0° to 70°:			Total time from 0° to 70°:		
M. S.			M. S.		
In Exp. N ^o 3. = 11 3			In Exp. N ^o 5. = 7 45		
In Exp. N ^o 4. = 10 34			In Exp. N ^o 6. = 7 25		
Medium = 10 48 $\frac{1}{2}$			Medium = 7 35		

It appears from these Experiments that the conducting power of air to that of the Torricellian vacuum, under the circumstances described, is as $7\frac{3}{8}$ to $10\frac{48\frac{1}{2}}{60}$ inversely, or as 1000 to 702 nearly; for the quantities of Heat communicated being equal, the intensity of the communication is as the times inversely.

In these Experiments the Heat passed through the surrounding medium into the bulb of the thermometer :

meter : in order to reverse the Experiment, and make the Heat pass *out of* the thermometer, I put the instruments into boiling water, and let them remain therein till they had acquired the temperature of the water ; that is to say, till the mercury in the inclosed thermometers stood at 80° ; and then, taking them out of the boiling water, I plunged them suddenly into a mixture of water and pounded ice, and moving them about continually in this mixture, I observed the times employed in cooling as follows :

Thermometer N ^o 1. Surrounded by a Torricellian vacuum. <i>Taken out of boiling water, and plunged into freezing water.</i>			Thermometer N ^o 2. Surrounded by air. <i>Taken out of boiling water, and plunged into freezing water.</i>		
Time elapsed.		Heat lost.	Time elapsed.		Heat lost.
Exp. N ^o 7.	Exp. N ^o 8.		Exp. N ^o 9.	Exp. N ^o 10.	
		80°			80°
M. S.	M. S.	°	M. S.	M. S.	°
1 2	0 54	70	0 33	0 33	70
0 58	1 2	60	0 39	0 34	60
1 17	1 18	50	0 44	0 44	50
1 46	1 37	40	0 55	0 55	40
2 5	2 16	30	1 17	1 18	30
3 14	3 10	20	1 57	1 57	20
5 42	5 59	10	3 44	3 40	10
Not observed.	Not observed.	0	40 10	Not observed.	0
Total time of cooling from 80° to 10° .			Total time of cooling from 80° to 10° .		
M. S.			M. S.		
In Exp. N ^o 7.=16 4			In Exp. N ^o 9.=9 49		
In Exp. N ^o 8.=16 16			In Exp. N ^o 10.=9 41		
Medium=16 10			Medium=9 45		

By

By these Experiments it appears, that the conducting power of air is to that of the Torricellian vacuum as $9\frac{1}{2}$ to $16\frac{1}{2}$ inverfely, or as 1000 to 603.

To determine whether the fame law would hold good when the heated thermometers, instead of being plunged into freezing water, were suffered to cool in the open air, I made the following Experiments. The thermometers N° 1 and N° 2 being again heated in boiling water, as in the laft Experiments, I took them out of the water, and fufpended them in the middle of a large room, where the air (which appeared to be perfectly at reft, the windows and doors being all fhut) was warm to the 16th degree of REAUMUR's thermometer, and the times of cooling were obferved as follows :

(Exp. N° 11.) Thermometer N° 1. Surrounded by a Torricellian vacuum. <i>Heated to 80°, and fufpended in the open air warm to 16°.</i>		(Exp. N° 12.) Thermometer N° 2. Surrounded by air. <i>Heated to 80°, and fufpended in the open air warm to 16°.</i>	
Time elapsed.	Heat loft.	Time elapsed.	Heat loft.
—	80°	—	80°
M. S.	°	M. S.	°
Not obferved.	70	Not obferved.	70
1 24	60	0 51	60
1 44	50	1 5	50
2 28	40	1 34	40
4 16	30	2 41	30
10 12 = total time employed in cooling from 70° to 30°.		6 11 = total time employed in cooling from 70° to 30°.	

Here the difference in the conducting powers of air and of the Torricellian vacuum appears to be

nearly the same as in the foregoing Experiments, being as $6\frac{1}{8}$ to $10\frac{1}{8}$ inversely, or as 1000 to 605. I could not observe the time of cooling from 80° to 70° , being at that time busied in suspending the instruments.

As it might possibly be objected to the conclusions drawn from these Experiments that, notwithstanding all the care that was taken in the constructing of the two instruments made use of that they should be perfectly alike, yet they might in reality be so far different either in shape or size, as to occasion a very sensible error in the result of the Experiments; to remove these doubts I made the following Experiments:

In the morning towards eleven o'clock, the weather being remarkably fine, the mercury in the barometer standing at 27 inches 11 lines, REAUMUR's thermometer at 15° , and the hygrometer at 47° , I repeated the Experiment N^o 3. (of heating the thermometer N^o 1 in boiling water, &c.) and immediately afterwards opened the cylinder containing the thermometer at its upper end, where it had been sealed, and letting the air into it, I re-sealed it hermetically, and repeated the Experiment again with the same instrument, the thermometer being now surrounded with air, like the thermometer N^o 2.

The result of these Experiments, which may be seen in the following Table, shews evidently, that the error arising from the difference of the shapes or dimensions of the two instruments in question was inconsiderable, if not totally imperceptible.

(Exp. N° 13.) Thermometer N° 1.		(Exp. N° 14.) The same Thermometer (N° 1.)	
Its bulb half an inch in diameter shut up in the centre of a glass globe 1 ¹ / ₂ inch in diameter, voided of air, and hermetically sealed.		The glass globe, containing the bulb of the thermometer, being now filled with air, and hermetically sealed.	
Taken out of freezing water, and plunged into boiling water.		Taken out of freezing water, and plunged into boiling water.	
Time elapsed.	Heat acquired.	Time elapsed.	Heat acquired.
	0°		0°
M. S.	0	M. S.	0
0 55	10	0 32	10
0 55	20	0 32	20
1 7	30	0 43	30
1 15	40	0 50	40
1 29	50	1 1	50
2 2	60	1 24	60
3 21	70	2 38	70
13 44	80	10 25	80
24 48 = total time of heating from 0° to 80°.		18 5 = total time of heating from 0° to 80°.	
Total time from 0° to 70° = 11' 4".		Total time from 0° to 70° = 7' 40".	

It appears, therefore, from these Experiments, that the conducting power of common atmospheric air is to that of the Torricellian vacuum as $7\frac{4}{80}$ to $11\frac{4}{80}$ inversely, or as 1000 to 602; which differs but very little from the result of all the foregoing Experiments.

Notwithstanding that it appeared, from the result of these last Experiments, that any difference there might possibly have been in the forms or dimensions of the instruments N° 1 and N° 2 could hardly have produced any sensible error in the result of the Experiments in question; I was willing, however, to see how far any con-

siderable alterations of size in the instrument would affect the Experiment : I therefore provided myself with another instrument which I shall call *Thermometer* N^o 3. different from those already described in size, and a little different in its construction.

The bulb of the thermometer was of the same form and size as in the instruments N^o 1 and N^o 2. that is to say, it was globular, and half an inch in diameter ; but the glass globe, in the centre of which it was confined, was much larger, being 3 inches $7\frac{1}{2}$ lines in diameter ; and the bore of the tube of the thermometer was much finer, and consequently its length, and the divisions of its scale, were greater. The divisions were marked upon the tube with threads of silk of different colours at every tenth degree, from 0° to 80°, as in the before-mentioned instruments. The tube or cylinder belonging to the glass globe was 8 lines in diameter, a little longer than the tube of the thermometer, and perfectly cylindrical from its upper end to its junction with the globe, being without any choak ; the thermometer being confined in the centre of the globe by a different contrivance, which was as follows. To the opening of the cylinder was fitted a stopple of dry wood, covered with a coating of hard varnish, through the centre or axis of which passed the end of the tube of the thermometer : this stopple confined the tube in the axis of the cylinder at its upper end. To confine it at its lower end, there was fitted to it a small steel spring, a little below the point 0° ; which, being fastened to the tube of the thermometer,

meter, had three elastic points projecting outwards, which, pressing against the inside of the cylinder, confined the thermometer in its place. The total length of this instrument, from the bottom of the globe to the upper end of the cylinder, was 18 inches, and the freezing point upon the thermometer fell about 3 inches above the bulb; consequently this point lay about $1\frac{1}{2}$ inch above the junction of the cylinder with the globe, when the thermometer was confined in its place, the centre of its bulb coinciding with the centre of the globe. Through the stopple which closed the end of the cylinder passed two small glass tubes, about a line in diameter, which being about a line longer than the stopple were closed occasionally with small stopples fitted to their bores. These tubes (which were fitted exactly in the holes bored in the great stopple of the cylinder to receive them, and fixed in their places with cement) served to convey air, or any other fluid, into the glass ball, without its being necessary to remove the stopple closing the end of the cylinder; which stopple, in order to prevent the position of the thermometer from being easily deranged, was cemented in its place.

I have been the more particular in the description of these instruments, as I conceive it to be absolutely necessary to have a perfect idea of them in order to judge of the Experiments made with them, and of their results.

With the instrument last described (which I have called *Thermometer* N^o 3.) I made the following

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Experi-

Experiment. It was upon the 18th of July 1785, in the afternoon, the weather variable, alternate clouds and sun-shine; wind strong at S. E. with now and then a sprinkling of rain; barometer at 27 inches $10\frac{1}{2}$ lines, thermometer at 18° , and hygrometer variable from 44° to extreme moisture.

In order to compare the result of the Experiment made with this instrument with those made with the thermometer N^o 2. I have placed together in the same Table the different Experiments made with them.

(Exp. N ^o 3.) Thermometer N ^o 3. Its bulb half an inch in diameter, shut up in the centre of a glass tube, 3 inches $7\frac{1}{2}$ lines in diameter, and surrounded by air. Taken out of freezing water, and plunged into boiling water.		(Exp. N ^o 4 and N ^o 5.) Thermometer N ^o 2. Its bulb half an inch in diameter, shut up in the centre of a glass globe, $1\frac{1}{2}$ inch in diameter, and surrounded by air. Taken out of freezing water, and plunged into boiling water.			
Time elapsed. Heat acquired.		Time elapsed.			Heat acquired.
		Exp. N ^o 4.	Exp. N ^o 5.	Medium.	
M. S.	°	M. S.	M. S.	M. S.	°
0 33	10	0 30	0 30	0 30	10
0 38	20	0 35	0 37	0 36	20
0 54	30	0 41	0 41	0 41	30
0 51	40	0 49	0 53	0 51	40
1 7	50	1 1	0 59	1 0	50
1 28	60	1 24	1 20	1 22	60
2 28	70	2 45	2 25	2 35	70
9 0	80	9 10	9 38	9 24	80
16 59 = total time of heating from 0° to 80° . Time from 0° to 70° = $7' 59''$.		16 55 17 3 16 50 = total time of heating from 0° to 80° . Time from 0° to 70° = $7' 35''$.			

If the agreement of these Experiments with the thermometers N^o 2 and N^o 3 surprised me, I was not less surprised with their disagreement in the Experiment which follows :

Experi-

Experiment, N° 16.

Taking the thermometer N° 3. out of the boiling water, I immediately suspended it in the middle of a large room, where the air, which was quiet, was at the temperature of $18^{\circ}\frac{1}{2}$ R. and observed the times of cooling as follows :

Time elapsed.	Heat lost.
—	80°
M. S.	°
1 55	70°
0 12	60°
0 33	50°
2 15	40°
4 0	30°
—	

9 55 = total time of cooling from 80° to 30° .

Time from 70° to 30° = $8' 0''$; but in the Experiment No. 12. with the thermometer N° 2. the time employed in cooling from 70° to 30° was only $6' 11''$. In this Experiment, with the thermometer N° 3. the time employed in cooling from 60° to 30° was $7' 48''$; but in the above-mentioned Experiment, with the thermometer N° 2. it was only $5' 20''$. It is true, the air of the room was somewhat cooler when the former Experiment was made, than when this latter was made, with the thermometer N° 3; but this difference of temperature, which was only $2^{\circ}\frac{1}{2}$, (in the former case the thermometer in the room standing at 16° , and in the latter at $18^{\circ}\frac{1}{2}$,) certainly could not have

occasioned the whole of the apparent difference in the results of the Experiments.

Does air receive Heat more readily than it parts with it? This is a question highly deserving of further investigation, and I hope to be able to give it a full examination in the course of my projected inquiries; but leaving it for the present, I shall proceed to give an account of the Experiments which I have already made. Conceiving it to be a step of considerable importance towards coming at a further knowledge of the nature of Heat, to ascertain, by indisputable evidence, its passage through the Torricellian vacuum, and to determine, with as much precision as possible, the law of its motions in that medium; and being apprehensive that doubts might arise with respect to the Experiments before described, on account of the contact of the tubes of the inclosed thermometers in the instruments made use of with the containing glass globes, or rather with their cylinders: by means of which (it might be suspected) that a certain quantity, if not all the Heat acquired, might possibly be communicated; to put this matter beyond all doubt, I made the following Experiment.

In the middle of a glass body, of a pear-like form, about 8 inches long, and $2\frac{1}{2}$ inches in its greatest diameter, I suspended a small mercurial thermometer, $5\frac{1}{2}$ inches long, by a fine thread of silk, in such a manner that neither the bulb of the thermometer, nor its tube, touched the containing glass body in any part. The tube of the thermometer

mometer was graduated, and marked with fine threads of silk of different colours, bound round it, as in the thermometers belonging to the other instruments already described; and the thermometer was suspended in its place by means of a small steel spring, to which the end of the thread of silk which held the thermometer being attached, it (the spring) was forced into a small globular protuberance or cavity, blown in the upper extremity of the glass body, about half an inch in diameter, where the spring remaining, the thermometer necessarily remained suspended in the axis of the glass body. There was an opening at the bottom of the glass body, through which the thermometer was introduced; and a barometrical tube being foldered to this opening, the inside of the glass body was voided of air by means of mercury; and this opening being afterwards sealed hermetically, and the barometrical tube being taken away, the thermometer was left suspended in a Torricellian vacuum.

In this instrument, as the inclosed thermometer did not touch the containing glass body in any part, on the contrary, being distant from its internal surface an inch or more in every part, it is clear, that whatever Heat passed *into* or *out of* the thermometer must have passed *through* the surrounding Torricellian vacuum: for it cannot be supposed, that the fine thread of silk, by which the thermometer was suspended, was capable of conducting any Heat at all, or at least any sensible quantity. I therefore flattered myself with hopes of being able,

able, with the assistance of this instrument, to determine positively with regard to the passage of Heat in the Torricellian vacuum: and this, I think, I have done, notwithstanding an unfortunate accident that put it out of my power to pursue the Experiments so far as I intended.

This instrument being fitted to a small stand or foot of wood, in such a manner that the glass body remained in a perpendicular situation, I placed it in my room, by the side of another inclosed thermometer (N^o 2.), which was surrounded by air, and observed the effects produced on it by the variation of Heat in the atmosphere. I soon discovered, by the motion of the mercury in the inclosed thermometer, that the Heat passed through the Torricellian vacuum; but it appeared plainly from the sluggishness, or great insensibility of the thermometer, that the Heat passed with much greater difficulty in this medium than in common air. I now plunged both the thermometers into a bucket of cold water; and I observed that the mercury in the thermometer surrounded by air descended much faster than that in the thermometer surrounded by the Torricellian vacuum. I took them out of the cold water, and plunged them into a vessel of hot water (having no conveniencies at hand to repeat the Experiment in due form with the freezing and with the boiling water); and the thermometer surrounded by the Torricellian vacuum appeared still to be much more insensible or sluggish than that surrounded by air.

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These trials were quite sufficient to convince me of the passage of Heat in the Torricellian vacuum, and also of the greater difficulty of its passage in that medium than in common air; but, not satisfied to rest my inquiries here, I took the first opportunity that offered, and set myself to repeat the Experiments which I had before made with the instruments N° 1 and N° 2. I plunged this instrument into a mixture of pounded ice and water, where I let it remain till the mercury in the inclosed thermometer had descended to 0°; when, taking it out of this cold mixture, I plunged it suddenly into a vessel of boiling water, and prepared myself to observe the ascent of the mercury in the inclosed thermometer, as in the foregoing Experiments; but unfortunately the moment the end of the glass body touched the boiling water, it cracked with the Heat at the point where it had been hermetically sealed, and the water rushing into the body, spoiled the Experiment: and I have not since had an opportunity of providing myself with another instrument to repeat it.

It having been my intention from the beginning to examine the conducting powers of the artificial airs or gasses, the thermometer N° 3. was constructed with a view to those Experiments; and having now provided myself with a stock of those different kinds of airs, I began with *fixed air*, with which, by means of water, I filled the globe and cylinder containing the thermometer; and stopping up the two holes in the great stopple closing the end of the cylinder, I exposed the instrument in
freezing

freezing water till the mercury in the inclosed thermometer had descended to 0° ; when, taking it out of the freezing water, I plunged it into a large vessel of boiling water, and prepared myself to observe the times of heating, as in the former cases; but an accident happened, which suddenly put a stop to the Experiment. Immediately upon plunging the instrument into the boiling water, the mercury began to rise in the thermometer with such uncommon celerity, that it had passed the first division upon the tube (which marked the 10th degree, according to REAUMUR's scale) before I was aware of its being yet in motion; and having thus missed the opportunity of observing the time elapsed when the mercury arrived at that point, I was preparing to observe its passage of the next, when all of a sudden the stopple closing the end of the cylinder was blown up the chimney with a great explosion, and the thermometer, which, being cemented to it by its tube, was taken along with it, and was broken to pieces, and destroyed in its fall.

This unfortunate Experiment, though it put a stop for the time to the inquiries proposed, opened the way to other researches not less interesting. Suspecting that the explosion was occasioned by the rarefaction of the water which remained attached to the inside of the globe and cylinder after the operation of filling them with fixed air; and thinking it more than probable, that the uncommon celerity with which the mercury rose in the thermometer was principally owing to the same cause,

cause, I was led to examine the conducting power of *moist air*, or air saturated with water.

For this Experiment I provided myself with a new thermometer N° 4. the bulb of which, being of the same form as those already described (*viz.* globular) was also of the same size, or half an inch in diameter. To receive this thermometer a glass cylinder was provided, 8 lines in diameter, and about 14 inches long, and terminated at one end by a globe $1\frac{1}{2}$ inch in diameter. In the centre of this globe the bulb of the thermometer was confined, by means of the stopple which closed the end of the cylinder; which stopple, being near 2 inches long, received the end of the tube of the thermometer into a hole bored through its centre or axis, and confined the thermometer in its place, without the assistance of any other apparatus. Through this stopple two other small holes were bored, and lined with thin glass tubes, as in the thermometer N° 3. opening a passage into the cylinder, which holes were occasionally stopped up with stopples of cork; but to prevent accidents, such as I have before experienced from an explosion, great care was taken not to press these stopples into their places with any considerable force, that they might the more easily be blown out by any considerable effort of the confined air, or vapour.

Though in this instrument the thermometer was not altogether so steady in its place as in the thermometers N° 1, N° 2, and N° 3. the elasticity of the tube, and the weight of the mercury in the bulb of the thermometer, occasioning a small vibration

vibration or trembling of the thermometer upon any sudden motion or jar; yet I preferred this method to the others, on account of the lower part of this thermometer being entirely free, or suspended in such a manner as not to touch, or have any communication with, the lower part of the cylinder or the globe: for though the quantity of Heat received by the tube of the thermometer at its contact with the cylinder at its choaks, in the instruments N^o 1 and N^o 2. or with the branches of the steel spring in N^o 3. and from thence communicated to the bulb, must have been exceedingly small; yet I was desirous to prevent even that, and every other possible cause of error or inaccuracy.

Does humidity augment the conducting power of air?

To determine this question I made the following Experiments, the weather being clear and fine, the mercury in the barometer standing at 27 inches 8 lines, the thermometer at 19°, and the hygrometer at 44°.

(Exp. N ^o 17.)		(Exp. N ^o 18.)	
Thermometer N ^o 4.		The same Thermometer (N ^o 4.)	
Surrounded by air <i>dry</i> to the 44th degree of the quill hygrometer of the Manheim Academy.		Surrounded by air rendered as <i>moist</i> as possible by wetting the inside of the cylinder and globe with water.	
Taken out of freezing water, and plunged into boiling water.		Taken out of freezing water, and plunged into boiling water.	
Time elapsed.	Heat acquired.	Time elapsed.	Heat acquired.
M. S.	80 ^o	M. S.	0 ^o
0 34	10	0 6	10
0 39	20	0 4	20
0 44	30	0 5	30
0 51	40	0 9	40
1 6	50	0 18	50
1 35	60	0 26	60
2 40	70	0 43	70
not observed.	80	7 45	80
8 9 = total time of heating from 0 ^o to 70 ^o .		1 51 = total time of heating from 0 ^o to 70 ^o .	

From these Experiments it appears, that the conducting power of air is very much increased by humidity. To see if the same result would obtain when the Experiment was reversed, I now took the thermometer with the *moist* air out of the boiling water, and plunged it into freezing water; and moving it about continually from place to place in the freezing water, I observed the times of cooling, as set down in the following Table. N. B. To compare the result of this Experiment with those made with *dry* air, I have placed on one side in the following Table the Experiment in question, and on the other side the Experiment N^o 19. made with the thermometer N^o 2.

(Exp. N° 19.) Thermometer N° 4. Surrounded by moist air. Taken out of boiling water, and plunged into freezing water.		(Exp. N° 10.) Thermometer N° 2. Surrounded by dry air. Taken out of boiling water, and plunged into freezing water.	
Time elapsed.	Heat lost.	Time elapsed.	Heat lost.
	80°		80°
M. S.	°	M. S.	°
0 4	70	0 33	70
0 14	60	0 34	60
0 31	50	0 44	50
0 52	40	0 55	40
1 22	30	1 18	30
2 3	20	1 57	20
4 2	10	3 40	10
9 8 = total time of cooling from 80° to 10°.		9 12 = total time of cooling from 80° to 10°.	

Though the difference of the whole times of cooling from 80° to 10° in these two Experiments appears to have been very small, yet the difference of the times taken up by the first twenty or thirty degrees from the boiling point is very remarkable, and shows with how much greater facility Heat passes in moist air than in dry air. Even the slowness with which the mercury in the thermometer N° 4. descended in this Experiment from the 30th to the 20th, and from the 20th to the 10th degree, I attribute in some measure to the great conducting power of the moist air with which it was surrounded; for the cylinder containing the thermometer and the moist air, being not wholly submerged in the freezing water, that part of it which remained out of the water was necessarily surrounded by the
air

air of the atmosphere ; which being much warmer than the water, communicated of its Heat to the glass ; which, passing from thence into the contained moist air as soon as that air became colder than the external air, was, through that medium, communicated to the bulb of the inclosed thermometer, which prevented its cooling so fast as it would otherwise have done. But when the weather becomes cold, I propose to repeat this Experiment with variations, in such a manner as to put the matter beyond all doubt. In the mean time I cannot help observing, with what infinite wisdom and goodness Divine Providence appears to have guarded us against the evil effects of excessive Heat and Cold in the atmosphere ; for if it were possible for the air to be equally damp during the severe cold of the winter months as it sometimes is in summer, its conducting power, and consequently its apparent coldness, when applied to our bodies, would be so much increased, by such an additional degree of moisture, that it would become quite intolerable ; but, happily for us, its power to hold water in solution is diminished, and with it its power to rob us of our animal heat, in proportion as its coldness is increased. Every body knows how very disagreeable a very moderate degree of cold is when the air is very damp ; and from hence it appears, why the thermometer is not always a just measure of the apparent or sensible Heat of the atmosphere. If colds or catarrhs are occasioned by our bodies being robbed of our animal heat, the reason is plain why those disorders prevail most during the

cold autumnal rains, and upon the breaking up of the frost in the spring. It is likewise plain from whence it is that sleeping in damp beds, and inhabiting damp houses, is so very dangerous; and why the evening air is so pernicious in summer and in autumn, and why it is not so during the hard frosts of winter. It has puzzled many very able philosophers and physicians to account for the manner in which the extraordinary degree or rather *quantity* of Heat is generated which an animal body is supposed to lose, when exposed to the cold of winter, above what it communicates to the surrounding atmosphere in warm summer weather; but is it not more than probable, that the difference of the quantities of Heat, actually lost or communicated, is infinitely less than what they have imagined? These inquiries are certainly very interesting; and they are undoubtedly within the reach of well contrived and well conducted Experiments. But taking my leave for the present of this curious subject of investigation, I hasten to the sequel of my Experiments.

Finding so great a difference in the conducting powers of common air and of the Torricellian vacuum, I was led to examine the conducting powers of common air of different degrees of density. For this Experiment I prepared the thermometer N° 4. by stopping up one of the small glass tubes passing through the stopple, and opening a passage into the cylinder, and by fitting a valve to the external overture of the other. The instrument, thus prepared, being put under the receiver of an
air-

air-pump, the air passed freely out of the globe and cylinder upon working the machine, but the valve above described prevented its return upon letting air into the receiver. The gage of the air-pump showed the degree of rarity of the air under the receiver, and consequently of that filling the globe and cylinder, and immediately surrounding the thermometer.

With this instrument, the weather being clear and fine, the mercury in the barometer standing at 27 inches 9 lines, the thermometer at 15° , and the hygrometer at 47° , I made the following Experiments.

(Exp. N ^o 20.) Thermometer N ^o 4. Surrounded by common air, barometer standing at 27 inches 9 lines. Taken out of freezing water, and plunged into boiling water.		(Exp. N ^o 21.) Thermometer N ^o 4. Surrounded by air rarefied by pumping till the barometer-gage stood at 6 inches $11\frac{1}{2}$ lines. Taken out of freezing water, and plunged into boiling water.		(Exp. N ^o 22.) Thermometer N ^o 4. Surrounded by air rarefied by pumping till the barometer-gage stood at 1 inch 2 lines. Taken out of freezing water, and plunged into boiling water.	
Time elapsed.	Heat acquired.	Time elapsed.	Heat acquired.	Time elapsed.	Heat acquired.
M. S.	0	M. S.	0	M. S.	0
0 31	10	0 31	10	0 29	10
0 40	20	0 38	20	0 36	20
0 41	30	0 44	30	0 49	30
0 47	40	0 51	40	1 1	40
1 4	50	1 7	50	1 1	50
1 25	60	1 19	60	1 24	60
2 28	70	2 27	70	2 31	70
10 17	80	10 21	80	not observed.	80
7 36 = total time of heating from 0 ^o to 70 ^o .		7 37 = total time of heating from 0 ^o to 70 ^o .		7 51 = total time of heating from 0 ^o to 70 ^o .	

The result of these Experiments, I confess, surprised me not a little; but the discovery of truth being

being the sole object of my inquiries (having no favourite theory to defend) it brings no disappointment along with it, under whatever unexpected shape it may appear. I hope that further Experiments may lead to the discovery of the cause why there is so little difference in the conducting powers of air of such very different degrees of rarity, while there is so great a difference in the conducting powers of air, and of the Torricellian vacuum. At present, I shall not venture any conjectures upon the subject; but in the mean time I dare to assert, that the Experiments I have made may be depended on.

The time of my stay at Manheim being expired (having had the honour to attend thither his most Serene Highness the Elector Palatine, reigning Duke of Bavaria, in his late journey), I was prevented from pursuing these inquiries further at that time; but I shall not fail to recommence them the first leisure moment I can find, which I fancy will be about the beginning of the month of November. In the mean time, to enable myself to pursue them with effect, I am sparing neither labour nor expence to provide a complete apparatus necessary for my purpose; and his Electoral Highness has been graciously pleased to order M. ARTARIA (who is in his service) to come to Munich to assist me. With such a Patron as his most Serene Highness, and with such an assistant as ARTARIA, I shall go on in my pursuits with chearfulness. Would to God that my labours might be

as useful to others as they will be pleasant to me!

I shall conclude this chapter with a short account of some Experiments I have made to determine the conducting powers of water and of mercury; and with a table, showing at one view the conducting powers of all the different mediums which I have examined.

Having filled the glass globe inclosing the bulb of the thermometer N° 4, first with water, and then with mercury, I made the following Experiments, to ascertain the conducting powers of those two Fluids.

(Exp. N° 23.) Thermometer N° 4. Surrounded by water. Taken out of freezing water, and plunged into boiling water.		(Exp. N° 24, 25, and 26.) Thermometer N° 4. Surrounded by mercury. Taken out of freezing water, and plunged into boiling water.			
Time elapsed. Heat acquired.		Time elapsed.			Heat acquired.
M. S.	°	Ex. N° 24.	Ex. N° 25.	Ex. N° 26.	°
0 19	10	0 5	0 5	0 5	10
0 3	20	0 4	0 2	0 5	20
0 9	30	0 2	0 2	0 4	30
0 11	40	0 4	0 5	0 5	40
0 15	50	0 4	0 4	0 7	50
0 21	60	0 7	0 4	0 8	60
0 34	70	0 15	0 9	0 14	70
2 13	80	Not observed.	0 58	Not observed.	80
1 57 = total time of heating from 0° to 70°.		0 41	0 31	0 48 = total times of heating from 0° to 70°.	

The total times of heating from 0° to 70° in the three Experiments with mercury being 41 seconds,
G G 3 31 seconds,

31 seconds, and 48 seconds, the mean of these times is $36\frac{2}{3}$ seconds; and as in the Experiment with water the time employed in acquiring the same degree of Heat was $1' 57'' = 117$ seconds, it appears from these Experiments, that the conducting power of mercury to that of water, under the circumstances described, is as $36\frac{2}{3}$ to 117 inversely, or as 1000 to 313. And hence it is plain, why mercury *appears* so much hotter, and so much colder, to the touch than water, when in fact it is of the same temperature: for the force or violence of the sensation of what appears *hot* or *cold* depends not entirely upon the temperature of the body exciting in us those sensations, or upon the degree of Heat it actually possesses, but upon the *quantity* of Heat it is capable of communicating to us, or receiving from us, in any given short period of time, or as the intensity of the communication; and this depends in a great measure upon the conducting powers of the bodies in question.

The sensation excited in us when we touch any thing that appears to us to be *hot* is the entrance of Heat into our bodies; that of *cold* is its exit; and whatever contributes to facilitate or accelerate this communication adds to the violence of the sensation. And this is another proof that the thermometer cannot be a just measure of the intensity of the *sensible* Heat, or Cold, existing in bodies; or rather, that the touch does not afford us a just indication of their *real* temperatures.

A TABLE of the CONDUCTING POWERS of the under-mentioned MEDIUMS as determined by the foregoing Experiments.

Therm. No 1.		Thermometer No 4.					
Taken out of freezing water, and plunged into boiling water.							
Time elapsed.							
Toricellian Vacuum (Exp. No 3, 4. and 13.)	Common air, density = 1, (Exp. No 20.)	Rarefied air, density = $\frac{1}{4}$ (Exp. No 21.)	Rarefied air, density = $\frac{1}{11}$ (Exp. No 22.)	Moist air (Exp. No 18.)	Water (Exp. No 23.)	Mercury (Exp. No 24, 25, and 26.)	Heat acquired.
M. S.	M. S.	M. S.	M. S.	M. S.	M. S.	M. S.	°
0 52	0 31	0 31	0 29	0 6	0 19	0 5	10
0 58	0 40	0 38	0 36	0 4	0 8	0 3	20
1 3	0 41	0 44	0 49	0 5	0 9	0 2	30
1 18	0 47	0 51	1 1	0 9	0 11	0 4	40
1 25	1 4	1 7	1 1	0 18	0 15	0 5	50
1 58	1 25	1 19	1 24	0 26	0 21	0 6	60
3 19	2 28	2 27	2 31	0 43	0 34	0 12	70
11 57	10 17	10 21	—	7 45	2 13	0 58	80
10 53	7 36	7 37	7 51	1 51	1 57	0 36 $\frac{1}{2}$	total
times of heating from 0° to 70°.							

In determining the relative conducting powers of these mediums, I have compared the times of the heating of the thermometers from 0° to 70° instead of taking the whole times from 0° to 80°, and this I have done on account of the small variation in the Heat of the boiling water arising from the variation of the weight of the atmosphere, and also on account of the very flow motion of the mercury between the 70th and the 80th degrees, and the difficulty of determining the precise mo-

ment when the mercury arrives at the 80th degree.

Taking now the conducting power of mercury = 1000, the conducting powers of the other mediums, as determined by these Experiments, will be as follows, *viz.*

Mercury	-	-	-	1000
Moist air	-	-	-	330
Water	-	-	-	313
Common air, density = 1				$80\frac{4}{100}$
Rarefied air, density = $\frac{1}{2}$				$80\frac{2}{100}$
Rarefied air, density = $\frac{1}{4}$				78
The Torricellian vacuum				55

And in these proportions are the quantities of Heat which these different mediums are capable of transmitting in any given time; and consequently these numbers express the relative *sensible* temperatures of the mediums, as well as their conducting powers. How far these decisions will hold good under a variation of circumstances experiment only can determine. This is certainly a subject of investigation not less curious in itself than it is interesting to mankind; and I wish that what I have done may induce others to turn their attention to this long neglected field of experimental inquiry. For my own part, I am determined not to quit it.

In the future prosecution of these inquiries, I do not mean to confine myself solely to the determining

ing of the conducting powers of Fluids; on the contrary, solids, and particularly such bodies as are made use of for cloathing, will be principal subjects of my future Experiments. I have indeed already begun these researches, and have made some progress in them; but I forbear to anticipate a matter which will be the subject of a future communication.

CHAP. II.

The relative Warmth of various Substances used in making artificial Cloathing, determined by Experiment.—Relative Warmth of Coverings of the same Thickness, and formed of the same Substance, but of different Densities.—Relative Warmth of Coverings formed of equal Quantities of the same Substance, disposed in different Ways.—Experiments made with a View to determining how far the Power which certain Bodies possess of confining Heat depends on their chemical Properties.—Experiments with Charcoal—with Lampblack—with Woodashes—Striking Experiments with Semen Lycopodii.—All these Experiments indicate that the Air which occupies the Interstices of Substances used in forming Coverings for confining Heat, acts a very important Part in that Operation.—Those Substances appear to prevent the Air from conducting the Heat.—An Inquiry concerning the Manner in which this is effected.—This Inquiry leads to a decisive Experiment from the Result of which it appears that Air is a perfect Non-conductor of Heat.—This Discovery affords the Means of explaining a Variety of interesting Phenomena in the Economy of Nature.

[Read before the ROYAL SOCIETY, January 19, 1792.]

THE confining and directing of Heat are objects of such vast importance in the œconomy of human life, that I have been induced to confine my researches

researches chiefly to those points, conceiving that very great advantages to mankind could not fail to be derived from the discovery of any new facts relative to these operations.

If the laws of the communication of Heat from one body to another were known, measures might be taken with certainty, in all cases, for confining it, and directing its operations, and this would not only be productive of great œconomy in the articles of fuel and clothing, but would likewise greatly increase the comforts and conveniencies of life; objects of which the philosopher should never lose sight.

The route which I have followed in this inquiry is that which I thought bid fairest to lead to useful discoveries. Without embarrassing myself with any particular theory, I have formed to myself a plan of experimental investigation, which I conceived would conduct me to the knowledge of *certain facts*, of which we are now ignorant, or very imperfectly informed, and with which it is of consequence that we should be made acquainted.

The first great object which I had in view in this enquiry was to ascertain, if possible, the cause of the warmth of certain bodies; or the circumstances upon which their power of confining Heat depends. This, in other words, is no other than to determine the cause of the conducting and non-conducting power of bodies, with regard to Heat.

To this end I began by determining by actual experiment the relative conducting powers of various

rious bodies of very different natures, both fluids and solids, of some of which Experiments I have already given an account in the Paper above mentioned, which is published in the Transactions of the Royal Society for the year 1786; I shall now, taking up the matter where I left it, give the continuation of the history of my researches.

Having discovered that the Torricellian vacuum is a much worse conductor of Heat than common air, and having ascertained the relative conducting powers of air, of water, and of mercury, under different circumstances, I proceeded to examine the conducting powers of various *solid bodies*, and particularly of such substances as are commonly made use of for clothing.

The method of making these Experiments was as follows: a mercurial thermometer, (see Fig. 4,) whose bulb was about $\frac{3}{16}$ of an inch in diameter, and its tube, about 10 inches in length, was suspended in the axis of a cylindrical glass tube, about $\frac{3}{4}$ of an inch in diameter, ending with a globe $\frac{1}{16}$ inch in diameter, in such a manner that the centre of the bulb of the thermometer occupied the centre of the globe; and the space between the internal surface of the globe and the surface of the bulb of the thermometer being filled with the substance whose conducting power was to be determined, the instrument was heated in boiling water, and afterwards being plunged into a freezing mixture of pounded ice and water, the times of cooling were observed, and noted down.

The

The tube of the thermometer was divided at every tenth degree from 0° , or the point of freezing, to 80° , that of boiling water, and these divisions being marked upon the tube with the point of a diamond, and the cylindrical tube being left empty, the height of the mercury in the tube of the thermometer was seen through it.

The thermometer was confined in its place by means of a stopple of cork, about $1\frac{1}{4}$ inch long, fitted to the mouth of the cylindrical tube, through the centre of which stopple the end of the tube of the thermometer passed, and in which it was cemented.

The operation of introducing into the globe the substances whose conducting powers are to be determined, is performed in the following manner; the thermometer being taken out of the cylindrical tube, about two-thirds of the substance which is to be the subject of the Experiment are introduced into the globe; after which, the bulb of the thermometer is introduced a few inches into the cylinder; and, after it, the remainder of the substance being placed round about the tube of the thermometer; and lastly, the thermometer being introduced farther into the tube, and being brought into its proper place, that part of the substance which, being introduced last, remains in the cylindrical tube above the bulb of the thermometer, is pushed down into the globe, and placed equally round the bulb of the thermometer by means of a brass wire which is passed through holes made for
that

that purpose in the stopple closing the end of the cylindrical tube.

As this instrument is calculated merely for measuring the passage of Heat in the substance whose conducting power is examined, I shall give it the name of *passage-thermometer*, and I shall apply the same appellation to all other instruments constructed upon the same principles, and for the same use, which I may in future have occasion to mention; and as this instrument has been so particularly described, both here, and in my former Paper upon the subject of Heat, in speaking of any others of the same kind in future it will not be necessary to enter into such minute details. I shall, therefore, only mention their *sizes*, or the diameters of their bulbs, the diameters of their globes, the diameters of their cylinders, and the lengths and divisions of their tubes, taking it for granted that this will be quite sufficient to give a clear idea of the instrument.

In most of my former Experiments, in order to ascertain the conducting power of any body, the body being introduced into the globe of the passage-thermometer, the instrument was cooled to the temperature of freezing water, after which, being taken out of the ice water, it was plunged suddenly into boiling water, and the times of heating from ten to ten degrees were observed and noted; and I said that these times were as the conducting power of the body inversely; but in the Experiments of which I am now about to give an account, I have in general reversed the operation; that is to say,
instead

instead of observing the times of heating, I have first heated the body in boiling water, and then plunging it into a mixture of pounded ice and ice-cold water, I have noted the times taken up in cooling.

I have preferred this last method to the former, not only on account of the greater ease and convenience with which a thermometer, plunged into a mixture of water, may be observed, than when placed in a vessel of boiling water, and surrounded by hot steam, but also on account of the greater accuracy of the Experiment, the heat of boiling water varying with the variations of the pressure of the atmosphere: consequently the Experiments made upon different days will have different results, and of course, strictly speaking, cannot be compared together; but the temperature of pounded ice and water is ever the same, and of course the results of the Experiments are uniform.

In heating the thermometer, I did not in general bring it to the temperature of the boiling water, as this temperature, as I have just observed, is variable; but when the mercury had attained the 75° of its scale, I immediately took it out of the boiling water, and plunged it into the ice and water; or, which I take to be still more accurate, suffering the mercury to rise a degree or two above 75° , and then taking it out of the boiling water, I held it over the vessel containing the pounded ice and water, ready to plunge it into that mixture the moment the mercury, descending, passes the 75° .

Having a watch at my ear which beat half seconds (which I counted), I noted the time of the passage

passage of the mercury over the divisions of the thermometer, marking 70° and every tenth degree from it, descending to 10° of the scale. I continued the cooling to 0° , or the temperature of the ice and water, in very few instances, as this took up much time, and was attended with no particular advantage, the determination of the times taken up in cooling 60 degrees of Reaumur's scale, that is to say, from 70° to 10° , being quite sufficient to ascertain the conducting power of any body whatever.

During the time of cooling in ice and water, the thermometer was constantly moved about in this mixture from one place to another; and there was always so much pounded ice mixed with the water, that the ice appeared above the surface of the water; the vessel, which was a large earthen jar, being first quite filled with pounded ice, and the water being afterwards poured upon it, and fresh quantities of pounded ice being added as the occasion required.

Having described the apparatus made use of in these Experiments, and the manner of performing the different operations, I shall now proceed to give an account of the Experiments themselves.

My first attempt was to discover the relative conducting powers of such substances as are commonly made use for clothing; accordingly, having procured a quantity of *raw silk*, as spun by the worm, *sheep's wool*, *cotton wool*, *linen* in the form of the finest lint, being the scrapings of very fine Irish linen, the finest part of the *fur of the beaver* separated from the skin, and from the long hair, the finest part of the *fur of a white Russian hare*, and
Eider

Eider down ; I introduced successively 16 grains in weight of each of these substances into the globe of the passage-thermometer, and placing it carefully and equally round the bulb of the thermometer, I heated the thermometer in boiling water, as before described, and taking it out of the boiling water, plunged it into pounded ice and water, and observed the times of cooling.

But as the interstices of these bodies thus placed in the globe were filled with air, I first made the Experiment with air alone, and took the result of that Experiment, as a standard by which to compare all the others ; the results of three Experiments with air were as follows :

The bulb of the thermometer surrounded by air.				
Heat lost.	Exp. No. 1.	Exp. No. 2.	Heat acquired.	Exp. No. 3.
	Time elapsed.	Time elapsed.		Time elapsed.
70°	—	—	10°	—
60°	38"	38"	20°	39"
50°	46	46	30°	43
40°	59	59	40°	53
30°	80	79	50°	67
20°	122	122	60°	96
10°	231	230	70°	175
Total times.	576	574	—	473

The following Table shows the results of the Experiments, with the various substances therein mentioned :

Heat lost.	Air.	Raw silk, 16 grs.	Sheep's wool, 16 grs.	Cotton wool, 16 grs.	Fine lint, 16 grs.	Beavers fur, 16 grs.	Hares fur, 16 grs.	Eider down, 16 grs.
	Exp. 1.	Exp. 4.	Exp. 5.	Exp. 6.	Exp. 7.	Exp. 8.	Exp. 9.	Ex. 10.
70°	—	—	—	—	—	—	—	—
60°	38"	94"	79"	83"	80"	99"	97"	98"
50°	46	110	95	95	93	116	117	116
40°	59	133	118	117	115	153	144	146
30°	80	185	162	152	150	185	193	192
20°	122	273	238	221	218	265	270	268
10°	231	489	426	378	376	478	494	485
Total times.	576	1284	1118	1046	1032	1296	1315	1305

Now the *warmth* of a body, or its power to confine Heat, being as its power of resisting the passage of Heat through it, (which I shall call its *non-conducting power*,) and the time taken up by any body in cooling, which is surrounded by any medium through which the Heat is obliged to pass, being, *ceteris paribus*, as the resistance which the medium opposes to the passage of the Heat, it appears that the *warmth* of the bodies mentioned in the foregoing Table are as the times of cooling; the *conducting powers* being inversely as those times, as I have formerly shown.

From the results of the foregoing Experiments it appears, that of the seven different substances made use of, hares fur and Eider down were the warmest;

warmest; after these came beavers fur; raw silk; sheep's wool; cotton wool; and lastly, lint, or the scrapings of fine linen; but I acknowledge that the differences in the warmth of these substances were much less than I expected to have found them.

Suspecting that this might arise from the volumes or solid contents of the substances being different, (though their weights were the same,) arising from the difference of their specific gravities; and as it was not easy to determine the specific gravities of these substances with accuracy, in order to see how far any known difference in the volume or quantity of the same substance, confined always in the same space, would add to or diminish the time of cooling, or the apparent warmth of the covering, I made the three following Experiments.

In the first, the bulb of the thermometer was surrounded by 16 grains of Eider down; in the second by 32 grains; and in the third by 64 grains; and in all these Experiments the substance was made to occupy exactly the same space, viz. the whole internal capacity of the glass globe, in the centre of which the bulb of the thermometer was placed; consequently the thickness of the covering of the thermometer remained the same, while its density was varied in proportion to the numbers 1, 2, and 4.

The results of these Experiments were as follow:

The bulb of the thermometer being furrounded by Eider down.			
Heat lost.	16 grains.	32 grains.	64 grains.
	(Exp. No. 11.)	(Exp. No. 12.)	(Exp. No. 13.)
70°	—	—	—
60°	97"	111"	112"
50°	117	128	130
40°	145	157	165
30°	192	207	224
20°	267	304	326
10°	486	565	658
Total times.	1304	1472	1615

Without stopping at present to draw any particular conclusions from the results of these Experiments, I shall proceed to give an account of some others, which will afford us a little further insight into the nature of some of the circumstances upon which the warmth of covering depends.

Finding, by the last Experiments, that the density of the covering added so considerably to the warmth of it, its thickness remaining the same, I was now desirous of discovering how far the internal structure of it contributed to render it more or less pervious to Heat, its thickness and quantity of matter remaining the same. By internal structure, I mean the disposition of the parts of the substance which forms the covering; thus they may be extremely divided, or very fine, as raw
filk

filk as spun by the worms, and they may be equally distributed through the whole space they occupy; or they may be coarser, or in larger masses, with larger interstices, as the ravelings of cloth, or cuttings of threads.

If Heat passed *through* the substances made use of for covering, and if the warmth of the covering depended solely upon the difficulty which the Heat meets with in its passage through the substances, *or solid parts*, of which they are composed; in that case, the warmth of covering would be always, *cæteris paribus*, as the quantity of materials of which it is composed; but that this is not the case, the following, as well as the foregoing Experiments clearly evince.

Having, in the Experiment N^o 4, ascertained the warmth of 16 grains of raw filk, I now repeated the Experiment with the same quantity, or weight, of the ravelings of white taffety, and afterwards with a like quantity of common sewing filk, cut into lengths of about two inches.

The following Table shows the results of these three Experiments:

Heat lost.	Raw silk, 16 grs.	Ravelins of taffety, 16 grs.	Sewing silk cut into lengths, 16 grs.
	Exp. 4.	Exp. 14.	Exp. 15.
70°	—	—	—
60°	94"	90"	67"
50°	110	106	79
40°	133	128	99
30°	185	172	135
20°	273	246	195
10°	489	427	342
Total times.	1284	1169	917

Here, notwithstanding that the quantities of the silk were the same in the three Experiments, and though in each of them it was made to occupy the same space, yet the warmth of the coverings which were formed were very different, owing to the different disposition of the material.

The raw silk was very fine, and was very equally distributed through the space it occupied, and it formed a warm covering.

The ravelins of taffety were also fine, but not so fine as the raw silk, and of course the interstices between its threads were greater, and it was less warm; but the cuttings of sewing silk were very coarse, and consequently it was very unequally distributed in the space in which it was confined; and it made a very bad covering for confining Heat.

It

It is clear from the results of the five last Experiments, that the air which occupies the interstices of bodies, made use of for covering, acts a very important part in the operation of confining Heat; yet I shall postpone the examination of that circumstance till I shall have given an account of several other Experiments, which, I think, will throw still more light upon that subject.

But, before I go any further, I will give an account of three Experiments which I made, or rather the same Experiment which I repeated three times the same day, in order to see how far they may be depended on, as being regular in their results.

The glass globe of the passage-thermometer being filled with 16 grains of cotton-wool, the instrument was heated and cooled three times successively, when the times of cooling were observed as follows :

Heat lost.	Exp. 16.	Exp. 17.	Exp. 18.
70°	—	—	—
60°	82"	84"	83"
50°	96	95	95
40°	118	117	116
30°	152	153	151
20°	221	221	220
10°	380	377	377
Total times.	1049	1047	1042

The difference of the times of cooling in these three Experiments were extremely small; but re-

gular as these Experiments appear to have been in their results, they were not more so than the other Experiments made in the same way, many of which were repeated two or three times, though, for the sake of brevity, I have put them down as single Experiments.

But to proceed in the account of my investigations relative to the causes of the warmth of warm clothing. Having found that the fineness and equal distribution of a body or substance made use of to form a covering to confine Heat, contributes so much to the warmth of the covering, I was desirous, in the next place to see the effect of condensing the covering, its quantity of matter remaining the same, but its thickness being diminished in proportion to the increase of its density.

The Experiment I made for this purpose was as follows:—I took 16 grains of common sewing silk, neither very fine nor very coarse, and winding it about the bulb of the thermometer in such a manner that it entirely covered it, and was as nearly as possible of the same thickness in every part, I replaced the thermometer in its cylinder and globe, and heating it in boiling water, cooled it in ice and water, as in the foregoing Experiments. The results of the Experiment were as may be seen in the following Table; and in order that it may be compared with those made with the same quantity of silk differently disposed of, I have placed those Experiments by the side of it :

Heat lost.	Raw silk, 16 grs.	Fine ravelings of taffety, 16 grs.	Sewing silk cut into lengths, 16 grs.	Sewing silk, 16 grs. wound round the bulb of the thermometer.
	Exp. No. 4.	Exp. No. 14.	Exp. No. 15.	Exp. No. 19.
70°	—	—	—	—
60°	94"	90"	67"	46"
50°	110	106	79	62
40°	133	128	99	85
30°	185	172	135	121
20°	273	246	195	191
10°	489	427	342	399
Total times.	1214	1169	917	904

It is not a little remarkable, that, though the covering formed of sewing silk wound round the bulb of the thermometer in the 19th Experiment, appeared to have so little power of confining the Heat when the instrument was very hot, or when it was first plunged into the ice and water, yet afterwards, when the Heat of the thermometer approached much nearer to that of the surrounding medium, its power of confining the Heat which remained in the bulb of the thermometer appeared to be even greater than that of the silk in the Experiment N° 15, the time of cooling from 20° to 10° being in the one 399", and in the other 342". The same appearance was observed in the following Experiments, in which the bulb of the thermometer was surrounded by threads of *wool*, of *cotton*, and of *linen*, or *flax*, wound round it, in the like manner

manner as the sewing filk was wound round it in the last Experiment.

The following Table shows the results of these Experiments, with the threads of various kinds; and that they may the more easily be compared with those made with the same quantity of the same substances in a different form, I have placed the accounts of these Experiments by the side of each other. I have also added the account of an Experiment, in which 16 grains of fine linen cloth were wrapped round the bulb of the thermometer, going round it nine times, and being bound together at the top and bottom of it, so as completely to cover it.

Heat lost.	<i>Sheep's wool, 16 grains, furrounding the bulb of the thermometer.</i>	<i>Woolen thread, 16 grains, wound round the bulb of the thermometer.</i>	<i>Cotton wool, 16 grains, furrounding the bulb of the thermometer.</i>	<i>Cotton thread, 16 grains, wound round the bulb of the thermometer.</i>	<i>Lint, 16 grains, furrounding the bulb of the thermometer.</i>	<i>Linen thread, 16 grains, wound round the bulb of the thermometer.</i>	<i>Linen cloth, 16 grains, wrapped round the bulb of the thermometer.</i>
	Exp. 5.	Ex. 20.	Exp. 6.	Ex. 21.	Exp. 7.	Ex. 22.	Exp. 23.
70°	—	—	—	—	—	—	—
60°	79"	46"	83"	45"	80"	46"	42"
50°	95	63	95	60	93	62	56
40°	118	89	117	83	115	83	74
30°	162	126	152	115	150	117	108
20°	238	200	221	179	218	180	168
10°	426	410	378	370	376	385	338
Total times.	1118	934	1046	852	1032	873	783

That thread wound light round the bulb of the thermometer should form a covering less warm than the same quantity of wool, or other raw materials

terials of which the thread is made, surrounding the bulb of the thermometer in a more loose manner, and consequently occupying a greater space, is no more than what I expected, from the idea I had formed of the causes of the warmth of covering; but I confess I was much surpris'd to find that there is so great a difference in the relative warmth of these two coverings, when they are employed to confine great degrees of Heat, and when the Heat they confine is much less in proportion to the temperature of the surrounding medium. This difference was very remarkable; in the Experiments with sheep's wool, and with woollen thread, the warmth of the covering formed of 16 grains of the former, was to that formed of 16 grains of the latter, when the bulb of the thermometer was heated to 70° and cooled to 60° , as 79 to 46 (the surrounding medium being at 0°); but afterwards, when the thermometer had only fallen from 20° to 10° of Heat, the warmth of the wool was to that of the woollen thread only as 426 to 410; and in the Experiments with lint, and with linen thread, when the Heat was much abated, the covering of the thread appeared to be even warmer than that of the lint, though in the beginning of the Experiments, when the Heat was much greater, the lint was warmer than the thread, in the proportion of 80 to 46.

From hence it should seem that a covering may, under certain circumstances, be very good for confining small degrees of warmth, which would be
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but very indifferent when made use of for confining a more intense Heat, and *vice versa*. This, I believe, is a new fact; and, I think the knowledge of it may lead to further discoveries relative to the causes of the warmth of coverings, or the manner in which Heat makes its passage through them. But I forbear to enlarge upon this subject, till I shall have given an account of several other Experiments, which I think throw more light upon it, and which will consequently render the investigation easier and more satisfactory.

With a view to determine how far the power which certain bodies appear to possess of confining Heat, when made use of as covering, depends upon the natures of those bodies, considered as chymical substances, or upon the chymical principles of which they are composed, I made the following Experiments.

As charcoal is supposed to be composed almost entirely of phlogiston, I thought that, if that principle was the cause either of the conducting power, or the non-conducting power of the bodies which contain it, I should discover it by making the Experiment with charcoal, as I had done with various other bodies. Accordingly, having filled the globe of the passage-thermometer with 176 grains of that substance in very fine powder, (it having been pounded in a mortar, and sifted through a fine sieve,) the bulb of the thermometer being surrounded by this powder, the instrument was heated in boiling water, and being afterwards plunged
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into a mixture of pounded ice and water, the times of cooling were observed as mentioned in the following Table. I afterwards repeated the Experiment with lampblack, and with very pure and very dry wood ashes; the results of which Experiments were as under-mentioned:

The bulb of the thermometer surrounded by				
Heat lost.	176 grains of fine powder of charcoal.	76 grains of fine powder of charcoal.	195 grains of lampblack.	307 grains of pure dry wood ashes.
	Exp. No. 24.	Exp. No. 25.	Exp. No. 26.	Exp. No. 27.
70°	—	—	—	—
60°	79"	91"	124"	96"
50°	95	91	118	92
40°	100	109	134	107
30°	139	133	164	136
20°	196	192	237	185
10°	331	321	394	311
Total times.	940	937	1171	927

The Experiment No. 25 was simply a repetition of that numbered 24, and was made immediately after it; but, in moving the thermometer about in the former Experiment, the powder of charcoal which filled the globe was shaken a little together, and to this circumstance I attribute the difference in the results of the two Experiments.

In the Experiments with lampblack and with wood ashes, the times taken up in cooling from 70° to 60° were greater than those employed in cooling from 60° to 50°; this most probably arose from

from the considerable quantity of Heat contained by these substances, which was first to be disposed of, before they could receive and communicate to the surrounding medium that which was contained by the bulb of the thermometer.

The next Experiment I made was with *semen lycopodii*, commonly called witch-meal, a substance which possesses very extraordinary properties. It is almost impossible to wet it; a quantity of it strewed upon the surface of a basin of water, not only swims upon the water without being wet, but it prevents other bodies from being wet which are plunged into the water through it; so that a piece of money, or other solid body, may be taken from the bottom of the basin by the naked hand, without wetting the hand; which is one of the tricks commonly shown by the jugglers in the country: this meal covers the hand, and descending along with it to the bottom of the basin, defends it from the water. This substance has the appearance of an exceeding fine, light, and very moveable yellow powder, and it is very inflammable; so much so, that being blown out of a quill into the flame of a candle, it flashes like gunpowder, and it is made use of in this manner in our theatres for imitating lightning.

Conceiving that there must have been a strong attraction between this substance and air, and suspecting, from some circumstances attending some of the foregoing Experiments, that the warmth of a covering depends not merely upon the fineness of the
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the substance of which the covering is formed, and the disposition of its parts, but that it arises in some measure from a certain attraction between the substance and the air which fills its interstices, I thought that an Experiment with *semen lycopodii* might possibly throw some light upon this matter; and in this opinion I was not altogether mistaken, as will appear by the results of the three following Experiments.

The bulb of the thermometer surrounded by 256 grs. of <i>semen lycopodii</i> .				
Heat lost.	Cooled.	Cooled.	Heat acquired.	Heated.
	Exp. No. 28.	Exp. No. 29.		Exp. No. 30.
70°	—	—	0°	—
60°	146"	157"	10°	230"
50°	162	160	20°	68
40°	175	170	30°	63
30°	209	203	40°	76
20°	284	288	50°	121
10°	502	513	60°	316
—	—	—	70°	1585
Total times.	1478	1491	—	2459

In the last Experiment (N° 30) the result of which was so very extraordinary, the instrument was cooled to 0° in thawing ice, after which it was plunged suddenly into boiling water, where it remained till the inclosed thermometer had acquired the Heat of 70°, which took up no less than 2456 seconds, or above 40 minutes; and it had remained

in the boiling water full a minute and an half before the mercury in the thermometer showed the least sign of rising. Having at length been put into motion, it rose very rapidly 40 or 50 degrees, after which its motion gradually abating became so slow, that it took up 1585 seconds, or something more than 26 minutes, in rising from 60° to 70° , though the temperature of the medium in which it was placed during the whole of this time was very nearly 80° ; the mercury in the barometer standing but little short of 27 Paris inches.

All the different substances which I had yet made use of in these Experiments for surrounding or covering the bulb of the thermometer, fluids excepted, had, in a greater, or in a less degree confined the Heat, or prevented its passing into or out of the thermometer so rapidly as it would have done, had there been nothing but air in the glass globe, in the centre of which the bulb of the thermometer was suspended. But the great question is, how, or in what manner, they produced this effect?

And first, it was not in consequence of their own non-conducting powers, simply considered; for, if instead of being only bad conductors of Heat, we suppose them to have been totally impervious to Heat, their volumes or solid contents were so exceedingly small in proportion to the capacity of the globe in which they were placed, that, had they had no effect whatever upon the air filling their interstices, that air would have been sufficient to have conducted all the Heat communicated, in less time than was actually taken up in the Experiment.

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The diameter of the globe being 1,6 inches, its contents amounted to 2,14466 cubic inches; and the contents of the bulb of the thermometer being only 0,08711 of a cubic inch, (its diameter being 0,55 of an inch,) the space between the bulb of the thermometer and the internal surface of the globe amounted to $2,14466 - 0,08711 = 2,05755$ cubic inches; the whole of which space was occupied by the substances by which the bulb of the thermometer was surrounded in the Experiments in question.

But though these substances occupied this space, they were far from *filling it*; by much the greater part of it being filled by the air which occupied the interstices of the substances in question. In the Experiment N° 4, this space was occupied by 16 grains of raw silk; and as the specific gravity of raw silk is to that of water as 1734 to 1000, the volume of this silk was equal to the volume of 9,4422 grains of water; and as 1 cubic inch of water weighs 253,185 grains, its volume was equal to $\frac{9,4422}{253,185} = 0,037294$ of a cubic inch; and, as the space it occupied amounted to 2,05755 cubic inches, it appears that the silk filled no more than about $\frac{1}{55}$ part of the space in which it was confined, the rest of that space being filled with air.

In the Experiment N° 1, when the space between the bulb of the thermometer and the glass globe, in the centre of which it was confined, was filled with nothing but air, the time taken up by the thermometer in cooling from 70° to 10° was

576 seconds; but in the Experiment N° 4, when this same space was filled with 54 parts air, and 1 part raw silk, the time of cooling was 1284 seconds.

Now, supposing that the silk had been totally incapable of conducting any Heat at all, if we suppose, at the same time, that it had no power to prevent the air remaining in the globe from conducting it, in that case its presence in the globe could only have prolonged the time of cooling in proportion to the quantity of the air it had displaced to the quantity remaining, that is to say, as 1 is to 54, or a little more than 10 seconds. But the time of cooling was actually prolonged 708 seconds (for in the Experiment N° 1, it was 576 seconds, and in the Experiment N° 4, it was 1284 seconds, as has just been observed); and this shows, that the silk not only did not conduct the Heat itself, but that it prevented the air by which its interstices were filled from conducting it; or, at least, it greatly weakened its power of conducting it.

The next question which arises is, how air can be prevented from conducting Heat? and this necessarily involves another, which is, how does air conduct Heat?

If air conducted Heat, as it is probable that the metals and water, and all other solid bodies and unelastic fluids conduct it, that is to say, if its particles remaining in their places, the Heat passed from one particle to another, through the whole mass,

mass, as there is no reason to suppose that the propagation of Heat is necessarily in right lines, I cannot conceive how the interposition of so small a quantity of any solid body as $\frac{1}{33}$ part of the volume of the air could have effected so remarkable a diminution of the conducting power of the air, as appeared in the Experiment (N^o 4) with raw silk, above mentioned.

If air and water conducted Heat in the same manner, it is more than probable that their conducting powers might be impaired by the same means; but when I made the Experiment with water, by filling the glass globe, in the centre of which the bulb of the thermometer was suspended, with that fluid, and afterwards varied the Experiment, by adding 16 grains of raw silk to the water, I did not find that the conducting power of the water was sensibly impaired by the presence of the silk *.

But we have just seen that the same silk, mixed with an equal volume of air, diminished its conducting power in a very remarkable degree; consequently, there is great reason to conclude that water and air conduct Heat in a *different manner*.

But the following Experiment, I think, puts the matter beyond all doubt.

* The Experiment here mentioned was made in the year 1787; but the result of a more careful investigation of the subject has since shown that Heat is not propagated in water in the manner here supposed. (See Essay VII.)

It is well known, that the power which air possesses of holding water in solution is augmented by Heat, and diminished by cold, and that, if hot air is saturated with water, and if this air is afterwards cooled, a part of its water is necessarily deposited.

I took a cylindrical bottle of very clear transparent glass, about 8 inches in diameter, and 12 inches high, with a short and narrow neck, and suspending a small piece of linen rag, moderately wet, in the middle of it, I plunged it into a large vessel of water, warmed to about 100° of Fahrenheit's thermometer, where I suffered it to remain till the contained air was not only warm, but thoroughly saturated with the moisture which it attracted from the linen rag, the mouth of the bottle being well stopped up during this time with a good cork; this being done, I removed the cork for a moment, to take away the linen rag, and stopping up the bottle again immediately, I took it out of the warm water, and plunged it into a large cylindrical jar, about 12 inches in diameter, and 16 inches high, containing just so much ice-cold water, that, when the bottle was plunged into it, and quite covered by it, the jar was quite full.

As the jar was of very fine transparent glass, as well as the bottle, and as the cold water contained in the jar was perfectly clear, I could see what passed in the bottle most distinctly; and having taken care to place the jar upon a table near the window,

window, in a very favourable light, I set myself to observe the appearances which should take place, with all that anxious expectation which a conviction that the result of the Experiment must be decisive, naturally inspired.

I was certain, that the air contained in the bottle could not part with its Heat, without at the same time, that is to say, *at the same moment, and in the same place*, parting with a portion of its water; if, therefore, the Heat penetrated the mass of air from the centre to the surface, or *passed through it* from particle to particle, in the same manner as it is probable that it passes through water, and all other unelastic fluids *, by far the greatest part of the air contained in the bottle would part with its Heat, when not *actually in contact with the glass*, and a proportional part of its water being let fall at the same time, and in the *same place*, would necessarily descend in the form of rain; and, though this rain might be too fine to be visible in its descent, yet I was sure I should find it at the bottom of the bottle, if not in visible drops of water, yet in that kind of cloudy covering which cold glass acquires from a contact with hot steam or watery vapour.

But if the particles of air, instead of communicating their Heat from one to another, from the centre to the surface of the bottle, each in its turn, and for itself, came to the surface of the bottle, and

* This opinion respecting the manner in which Heat is propagated in water, and other unelastic fluids, was afterwards found to be erroneous, as has been shown in the preceding Essay.

there deposited its Heat and its water, I concluded that the cloudiness occasioned by this deposit of water would appear all over the bottle, or, at least, not more of it at the bottom than at the sides, but rather less; and this I found to be the case in fact.

The cloudiness first made its appearance upon the sides of the bottle, near the top of it; and from thence it gradually spread itself downwards, till, growing fainter as it descended lower, it was hardly visible at the distance of half an inch from the bottom of the bottle; and upon the bottom itself, which was nearly flat, there was scarcely the smallest appearance of cloudiness.

These appearances, I think, are easy to be accounted for. The air immediately in contact with the glass being cooled, and having deposited a part of its water upon the surface of the glass, at the same time that it communicates to it its Heat, slides downwards by the sides of the bottle in consequence of its increased specific gravity, and, taking its place at the bottom of the bottle, forces the whole mass of hot air upwards; which, in its turn coming to the sides of the bottle, *there* deposits its Heat and its water, and afterwards bending its course downwards, this circulation is continued till all the air in the bottle has acquired the exact temperature of the water in the jar.

From hence it is clear why the first appearance of condensed vapour is near the top of the bottle, as also why the greatest collection of vapour is in
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that part, and that so very small a quantity of it is found nearer the bottom of the bottle.

This Experiment confirmed me in an opinion which I had for some time entertained, that, though the particles of air individually, or each for itself, are capable of receiving and *transporting* Heat, yet air in a quiescent state, or as a fluid whose parts are at rest with respect to each other, is not capable of conducting it, or giving it a passage; in short, that Heat is incapable of *passing through a mass of air*, penetrating from one particle of it to another, and that it is to this circumstance that its non-conducting power is principally owing.

It is also to this circumstance, in a great measure, that it is owing that its non-conducting power, or its apparent warmth when employed as a covering for confining Heat, is so remarkably increased upon its being mixed with a small quantity of any very fine, light, solid substance, such as the raw silk, fur, Eider down, &c. in the foregoing Experiments: for as I have already observed, though these substances, in the very small quantities in which they were made use of, could hardly have prevented, in any considerable degree, the air from conducting, or giving a *passage* to the Heat, had it been capable of passing through it, yet they might very much impede it in the operation of transporting it.

But there is another circumstance which it is necessary to take into the account, and that is the attraction which subsists between air and the bodies

above-mentioned, and other like substances, constituting natural and artificial clothing. For, though the incapacity of air to give a passage to Heat in the manner solid bodies permit it to pass through them, may enable us to account for its warmth under certain circumstances, yet the bare admission of this principle does not seem to be sufficient to account for the very extraordinary degrees of warmth which we find in furs and in feathers, and in various other kinds of natural and artificial clothing; nor even that which we find in snow; for if we suppose the particles of air to be at liberty to *carry off* the Heat which these bodies are meant to confine, without any other obstruction or hindrance than that arising from their *vis inertiae*, or the force necessary to put them in motion, it seems probable that the succession of fresh particles of cold air, and the consequent loss of Heat, would be much more rapid than we find it to be in fact.

That an attraction, and a very strong one, actually subsists between the particles of air, and the fine hair or furs of beasts, the feathers of birds, wool, &c. appears by the obstinacy with which these substances retain the air which adheres to them, even when immersed in water, and put under the receiver of an air-pump; and that this attraction is essential to the warmth of these bodies, I think is very easy to be demonstrated.

In furs, for instance, the attraction between the particles of air, and the fine hairs in which it is concealed, being greater than the increased elasticity,

ticity, or repulsion of those particles with regard to each other, arising from the Heat communicated to them by the animal body, the air in the fur, though heated, is not easily displaced; and this coat of confined air is the real barrier which defends the animal body from the external cold. This air cannot *carry off* the Heat of the animal, because it is itself confined, by its attraction to the hair or fur; and it transmits it with great difficulty, if it transmits at all, as has been abundantly shewn by the foregoing Experiments.

Hence it appears why those furs which are the finest, longest, and thickest, are likewise the warmest; and how the furs of the beaver, of the otter, and of other like quadrupeds which live much in water, and the feathers of water-fowls, are able to confine the Heat of those animals in winter, notwithstanding the extreme coldness and great conducting power of the water in which they swim. The attraction between these substances, and the air which occupies their interstices, is so great, that this air is not dislodged even by the contact of water, but remaining in its place, it defends the body of the animal at the same time from being wet, and from being robbed of its Heat by the surrounding cold fluid; and it is possible that the pressure of this fluid upon the covering of air confined in the interstices of the fur, or feathers, may at the same time increase its warmth, or non-conducting power, in such a manner that the animal may not, in fact, lose more Heat when in water, than
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when in air: for we have seen by the foregoing Experiments, that, under certain circumstances, the warmth of a covering is increased, by bringing its component parts nearer together, or by increasing its density even at the expence of its thickness. But this point will be further investigated hereafter.

Bears, wolves, foxes, hares, and other like quadrupeds, inhabitants of cold countries, which do not often take the water, have their fur much thicker upon their backs than upon their bellies. The heated air occupying the interstices of the hairs of the animal tending naturally to rise upwards, in consequence of its increased elasticity, would escape with much greater ease from the backs of quadrupeds than from their bellies, had not Providence wisely guarded against this evil by increasing the obstructions in those parts, which entangle it and confine it to the body of the animal. And this, I think, amounts almost to a proof of the principles assumed relative to the manner in which Heat is carried off by air, and the causes of the non-conducting power of air, or its apparent warmth, when, being combined with other bodies, it acts as a covering for confining Heat.

The snows which cover the surface of the earth in winter, in high latitudes, are doubtless designed by an all-provident Creator as a garment to defend it against the piercing winds from the polar regions, which prevail during the cold season.

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These winds, notwithstanding the vast tracts of continent over which they blow, retain their sharpness as long as the ground they pass over is covered with snow; and it is not till meeting with the ocean they acquire, from a contact with its waters, the Heat which the snows prevent their acquiring from the earth, that the edge of their coldness is taken off, and they gradually die away and are lost.

The winds are always found to be much colder when the ground is covered with snow than when it is bare, and this extraordinary coldness is vulgarly supposed to be communicated to the air by the snow; but this is an erroneous opinion; for these winds are in general much colder than the snow itself.

They retain their coldness, because the snow prevents them from being warmed at the expence of the earth; and this is a striking proof of the use of the snows in preserving the Heat of the earth during the winter in cold latitudes.

It is remarkable that these winds seldom blow from the poles directly towards the equator, but from the land towards the sea. Upon the eastern coast of North America the cold winds come from the north-west; but upon the western coast of Europe they blow from the north-east.

That they should blow towards those parts where they can most easily acquire the Heat they are in search of, is not extraordinary; and that they should gradually cease and die away, upon being warmed

warmed by a contact with the waters of the ocean, is likewise agreeable to the nature and causes of their motion; and if I might be allowed a conjecture respecting the principal use of the seas, or the reason why the proportion of water upon the surface of our globe is so great, compared to that of the land, it is to maintain a more equal temperature in the different climates, by heating or cooling the winds which at certain periods blow from the great continents.

That cold winds actually grow much milder upon passing over the sea, and that hot winds are refreshed by a contact with its waters, is very certain; and it is equally certain that the winds from the ocean are, in all climates, much more temperate than those which blow from the land.

In the islands of Great Britain and Ireland, there is not the least doubt but the great mildness of the climate is entirely owing to their separation from the neighbouring continent by so large a tract of sea; and in all similar situations, in every part of the globe, similar causes are found to produce similar effects.

The cold north-west winds, which prevail upon the coast of North America during the winter, seldom extend above 100 leagues from the shore, and they are always found to be less violent, and less piercing, as they are further from the land.

These periodical winds from the continents of Europe and North America prevail most towards the end of the month of February, and in the month
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of March; and I conceive that they contribute very essentially towards bringing on an early spring, and a fruitful summer, particularly when they are very violent in the month of March, and if at that time the ground is well covered with snow. The whole atmosphere of the polar regions being, as it were, transported into the ocean by these winds, is there warmed and saturated with water: and, a great accumulation of air upon the sea being the necessary consequence of the long continuance of these cold winds from the shore, upon their ceasing the warm breezes from the sea necessarily commence, and, spreading themselves upon the land far and wide, assist the returning sun in dismantling the earth of the remains of her winter garment, and in bringing forward into life all the manifold beauties of the new-born year.

This warmed air which comes in from the sea, having acquired its Heat from a contact with the ocean, is, of course, saturated with water; and hence the warm showers of April and May, so necessary to a fruitful season.

The ocean may be considered as the great reservoir and equalizer of Heat; and its benign influences in preserving a proper temperature in the atmosphere operate in all seasons and in all climates.

The parching winds from the land under the torrid zone are cooled by a contact with its waters, and, in return, the breezes from the sea, which at certain hours of the day come in to the shores in almost all hot countries, bring with them refreshment,

ment, and, as it were, new life and vigour both to the animal and vegetable creation, fainting and melting under the excessive Heats of a burning sun. What a vast tract of country, now the most fertile upon the face of the globe, would be absolutely barren and uninhabitable on account of the excessive Heat, were it not for these refreshing sea-breezes! And is it not more than probable, that the extremes of heat and of cold in the different seasons in the temperate and frigid zones would be quite intolerable, were it not for the influence of the ocean in preserving an equability of temperature?

And to these purposes the ocean is wonderfully well adapted not only on account of the great power of water to absorb Heat, and the vast depth and extent of the different seas (which are such that one summer or one winter could hardly be supposed to have any sensible effect in heating or cooling this enormous mass); but also on account of the continual circulation which is carried on in the ocean itself, by means of the currents which prevail in it. The waters under the torrid zone being carried by these currents towards the polar regions, are there cooled by a contact with the cold winds, and, having thus communicated their Heat to these inhospitable regions, return towards the equator, carrying with them refreshment for those parching climates.

The wisdom and goodness of Providence have often been called in question with regard to the distribution of land and water upon the surface of
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our globe, the vast extent of the ocean having been considered as a proof of the little regard that has been paid to man in this distribution. But, the more light we acquire respecting the real constitution of things, and the various uses of the different parts of the visible creation, the less we shall be disposed to indulge ourselves in such frivolous criticisms.

END OF THE EIGHTH ESSAY.

the globe, the vast extent of the ocean having
been considered as a great barrier to the
land part of the world, and the various
islands, we are to consider the various
changes of the sea and the various uses of the
ocean, and the various uses of the sea, and
shall be enabled to obtain a general view of the
state of the world.

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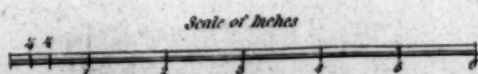
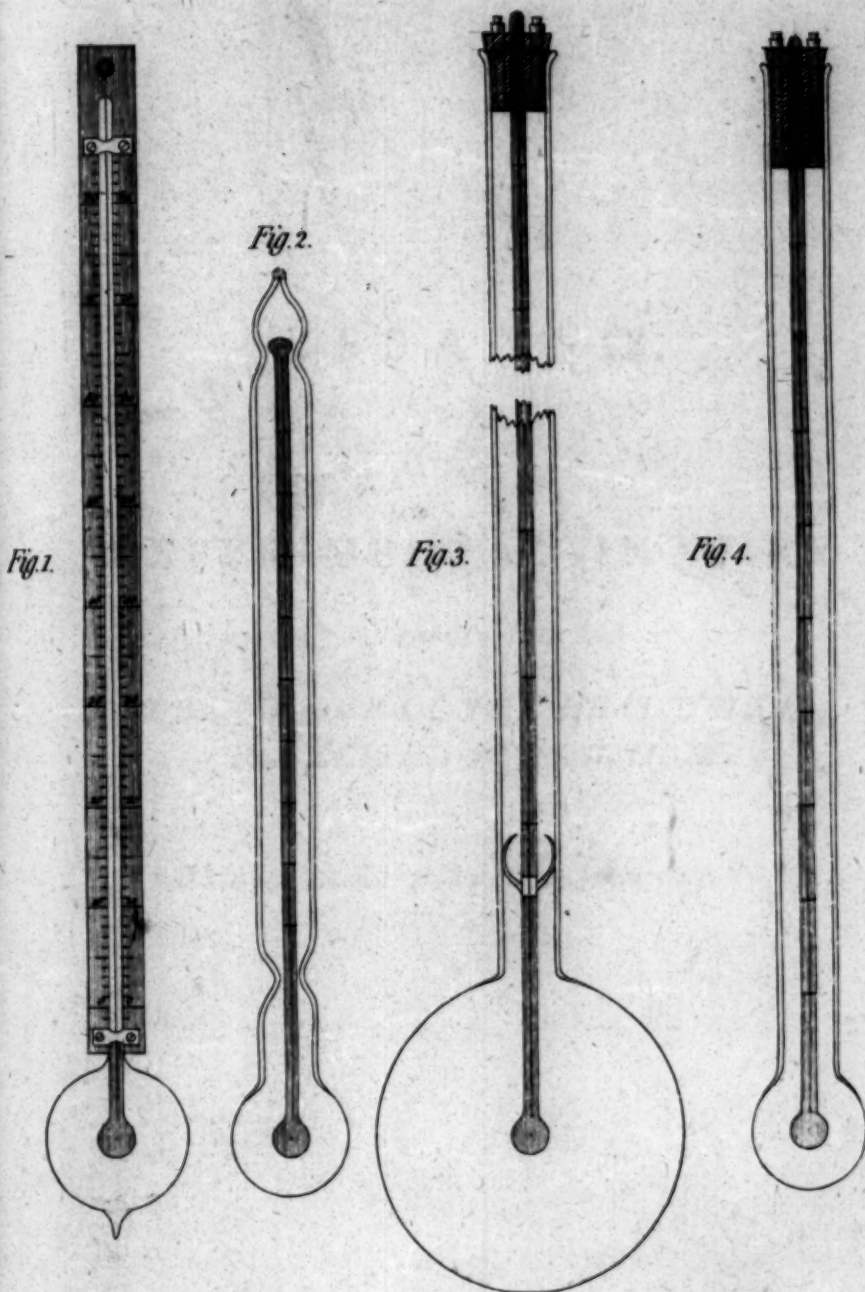
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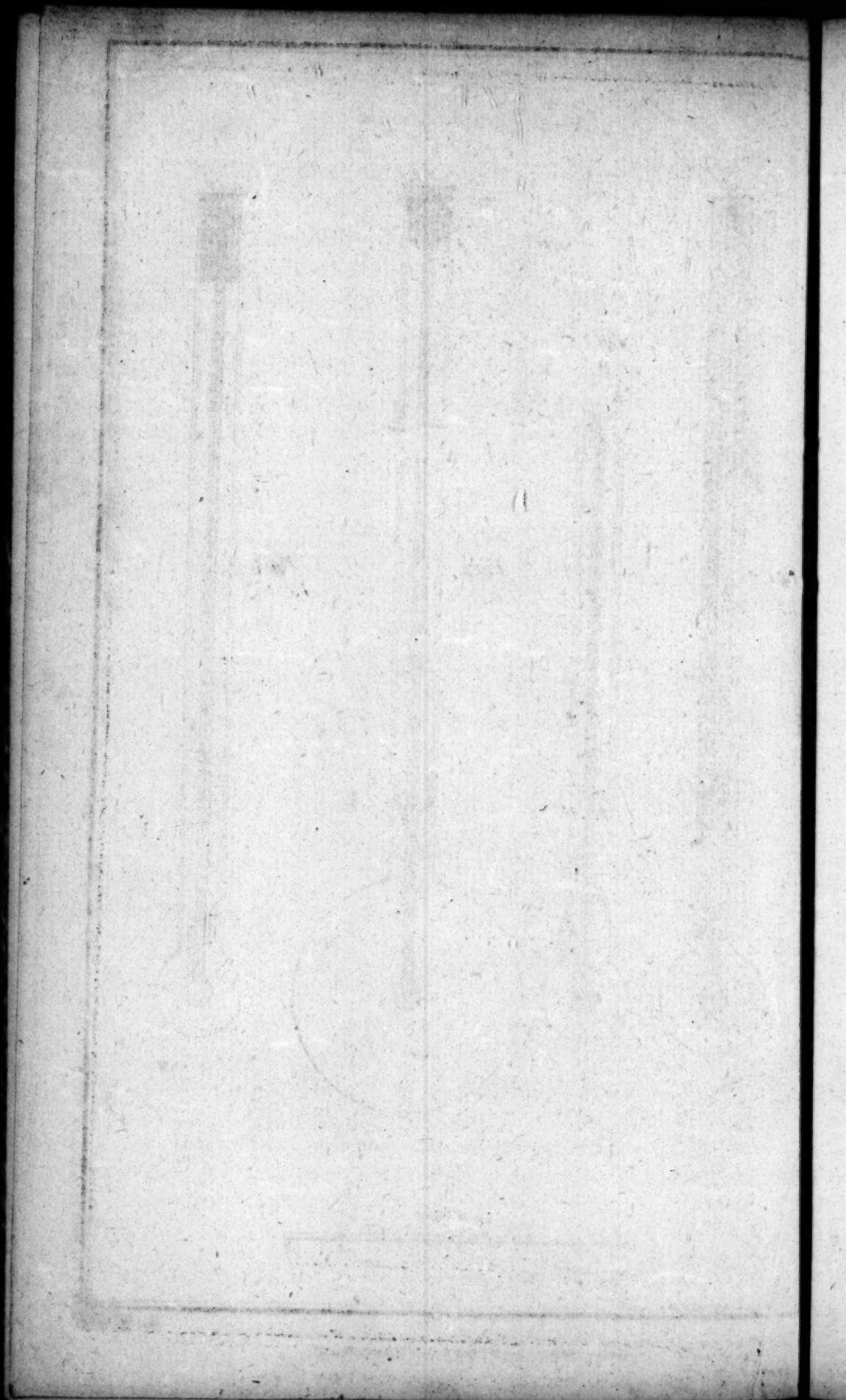
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ESSAY IX.

AN

EXPERIMENTAL INQUIRY

CONCERNING

*THE SOURCE OF THE HEAT WHICH
IS EXCITED BY FRICTION.*

[Read before the ROYAL SOCIETY, January 25, 1798.]

IX

EXPERIMENTAL INQUIRY

CONTENTS

THE SOURCE OF THE HEAT WHICH
CAUSES THE EXPLOSION

[Revised and corrected edition, 1902.]

E S S A Y IX.

An INQUIRY concerning the SOURCE of the
HEAT which is EXCITED by FRICTION.

[Read before the ROYAL SOCIETY, January 25, 1798.]

IT frequently happens, that in the ordinary affairs and occupations of life, opportunities present themselves of contemplating some of the most curious operations of Nature; and very interesting philosophical experiments might often be made, almost without trouble or expence, by means of machinery contrived for the mere mechanical purposes of the arts and manufactures.

I have frequently had occasion to make this observation; and am persuaded, that a habit of keeping the eyes open to every thing that is going on in the ordinary course of the business of life has oftener led, as it were by accident, or in the playful excursions of the imagination, put into action by contemplating the most common appearances, to useful doubts, and sensible schemes for investigation

and improvement, than all the more intense meditations of philosophers, in the hours expressly set apart for study.

It was by accident that I was led to make the Experiments of which I am about to give an account ; and, though they are not perhaps of sufficient importance to merit so formal an introduction, I cannot help flattering myself that they will be thought curious in several respects, and worthy of the honour of being made known to the Royal Society.

Being engaged, lately, in superintending the boring of cannon, in the workshops of the military arsenal at Munich, I was struck with the very considerable degree of Heat which a brass gun acquires, in a short time, in being bored ; and with the still more intense Heat (much greater than that of boiling water, as I found by experiment) of the metallic chips separated from it by the borer.

The more I meditated on these phænomena, the more they appeared to me to be curious and interesting. A thorough investigation of them seemed even to bid fair to give a farther insight into the hidden nature of Heat ; and to enable us to form some reasonable conjectures respecting the existence, or non-existence, of an *igneous fluid* : a subject on which the opinions of philosophers have, in all ages, been much divided.

In order that the Society may have clear and distinct ideas of the speculations and reasonings to which these appearances gave rise in my mind, and also of the specific objects of philosophical investigation

gation they suggested to me, I must beg leave to state them at some length, and in such manner as I shall think best suited to answer this purpose.

From *whence comes* the Heat actually produced in the mechanical operation above mentioned?

Is it furnished by the metallic chips which are separated by the borer from the solid mass of metal?

If this were the case, then, according to the modern doctrines of latent Heat, and of caloric, the *capacity for Heat* of the parts of the metal, so reduced to chips, ought not only to be changed, but the change undergone by them should be sufficiently great to account for *all* the Heat produced.

But no such change had taken place; for I found, upon taking equal quantities, by weight, of these chips, and of thin slips of the same block of metal separated by means of a fine saw, and putting them, at the same temperature, (that of boiling water,) into equal quantities of cold water, (that is to say, at the temperature of $59^{\circ}\frac{1}{2}$ F.) the portion of water into which the chips were put was not, to all appearance, heated either less or more than the other portion, in which the slips of metal were put.

This Experiment being repeated several times, the results were always so nearly the same, that I could not determine whether any, or what change, had been produced in the metal, *in regard to its*

capacity for Heat, by being reduced to chips by the borer *.

From hence it is evident, that the Heat produced could not possibly have been furnished at the expence of the latent Heat of the metallic chips. But, not being willing to rest satisfied with these trials, however conclusive they appeared to me to be, I had recourse to the following still more decisive Experiment :

Taking a cannon, (a brass six-pounder,) cast solid, and rough as it came from the foundry, (see Fig. 1. Tab. IV.) and fixing it (horizontally) in

* As these Experiments are important, it may perhaps be agreeable to the Society to be made acquainted with them in their details.

One of them was as follows :

To 4590 grains of water, at the temperature of $59^{\circ}\frac{1}{2}$ F. (an allowance as compensation, reckoned in water, for the capacity for Heat of the containing cylindrical tin vessel, being included,) were added 1016 $\frac{1}{2}$ grains of gun-metal in thin slips, separated from the gun by means of a fine saw, being at the temperature of 210° F. When they had remained together 1 minute, and had been well stirred about, by means of a small rod of light wood, the Heat of the mixture was found to be $= 63^{\circ}$.

From this Experiment, the *specific Heat* of the metal, calculated according to the rule given by Dr. CRAWFORD, turns out to be $= 0.1100$, that of water being $= 1.0000$.

An Experiment was afterwards made with the metallic chips, as follows :

To the same quantity of water as was used in the Experiment above mentioned, at the same temperature, (*viz.* $59^{\circ}\frac{1}{2}$), and in the same cylindrical tin vessel, were now put 1016 $\frac{1}{2}$ grains of metallic chips of gun-metal, bored out of the same gun from which the slips used in the foregoing Experiment were taken, and at the same temperature (210°). The Heat of the mixture, at the end of 1 minute, was just 63° , as before; consequently the *specific Heat* of these metallic chips was $= 0.1100$. Each of the above Experiments was repeated three times, and always with nearly the same results.

the

the machine used for boring, and at the same time finishing the outside of the cannon by turning, (see Fig. 2.) I caused its extremity to be cut off; and, by turning down the metal in that part, a solid cylinder was formed, $7\frac{3}{4}$ inches in diameter, and $9\frac{8}{16}$ inches long; which, when finished, remained joined to the rest of the metal (that which, properly speaking, constituted the cannon) by a small cylindrical neck, only $2\frac{1}{3}$ inches in diameter, and $3\frac{8}{16}$ inches long.

This short cylinder, which was supported in its horizontal position, and turned round its axis, by means of the neck by which it remained united to the cannon, was now bored with the horizontal borer used in boring cannon; but its bore, which was 3.7 inches in diameter, instead of being continued through its whole length (9.8 inches) was only 7.2 inches in length; so that a solid bottom was left to this hollow cylinder, which bottom was 2.6 inches in thickness.

This cavity is represented by dotted lines in Fig. 2; as also in Fig. 3. where the cylinder is represented on an enlarged scale.

This cylinder being designed for the express purpose of generating Heat *by friction*, by having a blunt borer forced against its solid bottom at the same time that it should be turned round its axis by the force of horses, in order that the Heat accumulated in the cylinder might from time to time be measured, a small round hole, (see *d, e*, Fig. 3.) 0.37 of an inch only in diameter, and 4.2 inches in

depth, for the purpose of introducing a small cylindrical mercurial thermometer, was made in it, on one side, in a direction perpendicular to the axis of the cylinder, and ending in the middle of the solid part of the metal which formed the bottom of its bore.

The solid contents of this hollow cylinder, exclusive of the cylindrical neck by which it remained united to the cannon, were $385\frac{1}{2}$ cubic inches, English measure; and it weighed 113.13lb. Avoirdupois: as I found, on weighing it at the end of the course of Experiments made with it, and after it had been separated from the cannon with which, during the Experiments, it remained connected*.

Experiment, N° 1.

This Experiment was made in order to ascertain how much Heat was actually generated by friction, when a blunt steel borer being so forcibly shoved (by means of a strong screw) against the bottom of

* For fear I should be suspected of prodigality in the prosecution of my philosophical researches, I think it necessary to inform the Society, that the cannon I made use of in this Experiment was not sacrificed to it. The short hollow cylinder which was formed at the end of it, was turned out of a cylindrical mass of metal, about 2 feet in length, projecting beyond the muzzle of the gun, called in the German language the *werlerner kopf*, (the head of the cannon to be thrown away,) and which is represented in Fig. 1.

This original projection, which is cut off before the gun is bored, is always cast with it, in order that, by means of the pressure of its weight on the metal in the lower part of the mould, during the time it is cooling, the gun may be the more compact in the neighbourhood of the muzzle; where, without this precaution, the metal would be apt to be porous, or full of honeycombs.

the

the bore of the cylinder, that the pressure against it was equal to the weight of about 10000 lb. Avoirdupois, the cylinder was turned round on its axis (by the force of horses) at the rate of about 32 times in a minute.

This machinery, as it was put together for the Experiment, is represented by Fig. 2. *W* is a strong horizontal iron bar, connected with proper machinery carried round by horses, by means of which the cannon was made to turn round its axis.

To prevent, as far as possible, the loss of any part of the Heat that was generated in the Experiment, the cylinder was well covered up with a fit coating of thick and warm flannel, which was carefully wrapped round it, and defended it on every side from the cold air of the atmosphere. This covering is not represented in the drawing of the apparatus, Fig. 2.

I ought to mention, that the borer was a flat piece of hardened steel, 0.63 of an inch thick, 4 inches long, and nearly as wide as the cavity of the bore of the cylinder, namely, $3\frac{1}{2}$ inches. Its corners were rounded off at its end, so as to make it fit the hollow bottom of the bore; and it was firmly fastened to the iron bar (*m*) which kept it in its place. The area of the surface by which its end was in contact with the bottom of the bore of the cylinder was nearly $2\frac{1}{3}$ inches. This borer, which is distinguished by the letter *n*, is represented in most of the figures.

At

At the beginning of the Experiment, the temperature of the air in the shade, as also that of the cylinder, was just 60° F.

At the end of 30 minutes, when the cylinder had made 960 revolutions about its axis, the horses being stopped, a cylindrical mercurial thermometer, whose bulb was $\frac{3}{8}$ of an inch in diameter, and $3\frac{1}{4}$ inches in length, was introduced into the hole made to receive it, in the side of the cylinder, when the mercury rose almost instantly to 130°.

Though the Heat could not be supposed to be quite equally distributed in every part of the cylinder, yet, as the length of the bulb of the thermometer was such that it extended from the axis of the cylinder to near its surface, the Heat indicated by it could not be very different from that of the *mean temperature* of the cylinder; and it was on this account that a thermometer of that particular form was chosen for this Experiment.

To see how fast the Heat escaped out of the cylinder, (in order to be able to make a probable conjecture respecting the quantity given off by it, during the time the Heat generated by the friction was accumulating,) the machinery standing still, I suffered the thermometer to remain in its place near three quarters of an hour, observing and noting down, at small intervals of time, the height of the temperature indicated by it.

Thus,

Thus, at the end of

The Heat, as shown by
the thermometer, was

4 minutes	- - - - -	126°
after 5 minutes, always reckon-		
ing from the first ob-		
servation,	- - - - -	125°
at the end of 7 minutes	- - - - -	123°
12 ———	- - - - -	120°
14 ———	- - - - -	119°
16 ———	- - - - -	118°
20 ———	- - - - -	116°
24 ———	- - - - -	115°
28 ———	- - - - -	114°
31 ———	- - - - -	113°
34 ———	- - - - -	112°
37½ ———	- - - - -	111°
and when 41 minutes had elapsed	-	110°

Having taken away the borer, I now removed the metallic dust, or rather scaly matter, which had been detached from the bottom of the cylinder by the blunt steel borer, in this Experiment; and, having carefully weighed it, I found its weight to be 837 grains Troy.

Is it possible that the very considerable quantity of Heat that was produced in this Experiment (a quantity which actually raised the temperature of above 113lb. of gun-metal at least 70 degrees of FAHRENHEIT's thermometer, and which, of course, would have been capable of melting 6½ lb. of ice, or of causing near 5lb. of ice-cold water to boil) could

could have been furnished by so inconsiderable a quantity of metallic dust? and this merely in consequence of a *change* of its capacity for Heat?

As the weight of this dust (837 grains Troy) amounted to no more than $\frac{1}{848}$ th part of that of the cylinder, it must have lost no less than 948 degrees of Heat, to have been able to have raised the temperature of the cylinder 1 degree; and consequently it must have given off 66360 degrees of Heat, to have produced the effects which were actually found to have been produced in the Experiment!

But, without insisting on the improbability of this supposition, we have only to recollect, that from the results of actual and decisive Experiments, made for the express purpose of ascertaining that fact, the capacity for Heat, of the metal of which great guns are cast, *is not sensibly changed* by being reduced to the form of metallic chips, in the operation of boring cannon; and there does not seem to be any reason to think that it can be much changed, if it be changed at all, in being reduced to much smaller pieces, by means of a borer that is less sharp.

If the Heat, or any considerable part of it, were produced in consequence of a change in the capacity for Heat of a part of the metal of the cylinder, as such change could only be *superficial*, the cylinder would by degrees be *exhausted*; or the quantities of Heat produced, in any given short space of time, would be found to diminish gradually, in
successive

successive Experiments. To find out if this really happened or not, I repeated the last-mentioned Experiment several times, with the utmost care; but I did not discover the smallest sign of exhaustion in the metal, notwithstanding the large quantities of Heat actually given off.

Finding so much reason to conclude, that the Heat generated in these Experiments, or *excited*, as I would rather choose to express it, was not furnished *at the expence of the latent Heat or combined caloric* of the metal, I pushed my inquiries a step farther, and endeavoured to find out whether the air did, or did not, contribute any thing in the generation of it.

Experiment, N° 2.

As the bore of the cylinder was cylindrical, and as the iron bar (*m*), to the end of which the blunt steel borer was fixed, was square. the air had free access to the inside of the bore, and even to the bottom of it, where the friction took place by which the Heat was excited.

As neither the metallic chips produced in the ordinary course of the operation of boring brass cannon, nor the finer scaly particles produced in the last-mentioned Experiments by the friction of the blunt borer, showed any signs of calcination, I did not see how the air could possibly have been the cause of the Heat that was produced; but, in an investigation of this kind, I thought that no pains should be spared to clear away the rubbish,
I and

and leave the subject as naked and open to inspection as possible.

In order, by one decisive Experiment, to determine whether the air of the atmosphere had any part, or not, in the generation of the Heat, I contrived to repeat the Experiment, under circumstances in which *it was evidently impossible for it to produce any effect whatever*. By means of a piston exactly fitted to the mouth of the bore of the cylinder, through the middle of which piston the square iron bar, to the end of which the blunt steel borer was fixed, passed in a square hole made perfectly air-tight, the access of the external air, to the inside of the bore of the cylinder, was effectually prevented. (In Fig. 3. this piston (*p*) is seen in its place; it is likewise shown in Fig. 7 and 8.).

I did not find, however, by this Experiment, that the exclusion of the air diminished, in the smallest degree, the quantity of Heat excited by the friction.

There still remained one doubt, which, though it appeared to me to be so slight as hardly to deserve any attention, I was however desirous to remove. The piston which closed the mouth of the bore of the cylinder, in order that it might be air-tight, was fitted into it with so much nicety, by means of its collars of leather, and pressed against it with so much force, that, notwithstanding its being oiled, it occasioned a considerable degree of friction, when the hollow cylinder was turned round

round its axis. Was not the Heat produced, or at least some part of it, occasioned by this friction of the piston? and, as the external air had free access to the extremity of the bore, where it came in contact with the piston, is it not possible that this air may have had some share in the generation of the Heat produced?

Experiment, N^o 3.

A quadrangular oblong deal box, (see Fig. 4.) water-tight, $11\frac{1}{2}$ English inches long, $9\frac{1}{8}$ inches wide, and $9\frac{6}{8}$ inches deep, (measured in the clear,) being provided, with holes or slits in the middle of each of its ends, just large enough to receive, the one, the square iron rod to the end of which the blunt steel borer was fastened, the other, the small cylindrical neck which joined the hollow cylinder to the cannon; when this box (which was occasionally closed above, by a wooden cover or lid moving on hinges) was put into its place; that is to say, when, by means of the two vertical openings or slits in its two ends, (the upper parts of which openings were occasionally closed, by means of narrow pieces of wood sliding in vertical grooves,) the box (*g, h, i, k*, Fig. 3.) was fixed to the machinery, in such a manner that its bottom (*i, k*), being in the plane of the horizon, its axis coincided with the axis of the hollow metallic cylinder; it is evident, from the description, that the hollow metallic cylinder would occupy the middle of the box, without touching it on either side (as it is represented

fented in Fig. 3.) ; and that, on pouring water into the box, and filling it to the brim, the cylinder would be completely covered, and surrounded on every side, by that fluid. And farther, as the box was held fast by the strong square iron rod (*m*), which passed, in a *square hole*, in the centre of one of its ends, (*a*, Fig. 4.) while the round or cylindrical neck, which joined the hollow cylinder to the end of the cannon, could turn round freely on its axis in the *round hole* in the centre of the other end of it, it is evident that the machinery could be put in motion, without the least danger of forcing the box out of its place, throwing the water out of it, or deranging any part of the apparatus.

Every thing being ready, I proceeded to make the Experiment I had projected, in the following manner:

The hollow cylinder having been previously cleaned out, and the inside of its bore wiped with a clean towel till it was quite dry, the square iron bar, with the blunt steel borer fixed to the end of it, was put into its place ; the mouth of the bore of the cylinder being closed at the same time, by means of the circular piston, through the centre of which the iron bar passed.

This being done, the box was put in its place, and the joinings of the iron rod, and of the neck of the cylinder, with the two ends of the box, having been made water-tight, by means of collars of oiled leather, the box was filled with cold water, (*viz.* at the temperature of 60° .) and the machine was put in motion.

The

The result of this beautiful Experiment was very striking, and the pleasure it afforded me amply repaid me for all the trouble I had had, in contriving and arranging the complicated machinery used in making it.

The cylinder, revolving at the rate of about 32 times in a minute, had been in motion but a short time, when I perceived, by putting my hand into the water, and touching the outside of the cylinder, that Heat was generated; and it was not long before the water which surrounded the cylinder began to be sensibly warm.

At the end of 1 hour I found, by plunging a thermometer into the water in the box, (the quantity of which fluid amounted to 18.77 lb. Avoirdupois, or $2\frac{1}{4}$ wine gallons,) that its temperature had been raised no less than 47 degrees; being now 107° of FAHRENHEIT's scale.

When 30 minutes more had elapsed, or 1 hour and 30 minutes after the machinery had been put in motion, the Heat of the water in the box was 142° .

At the end of 2 hours, reckoning from the beginning of the Experiment, the temperature of the water was found to be raised to 178° .

At 2 hours 20 minutes it was at 200° ; and at 2 hours 30 minutes it ACTUALLY BOILED!

It would be difficult to describe the surprise and astonishment expressed in the countenances of

the by-standers, on seeing so large a quantity of cold water heated, and actually made to boil, without any fire.

Though there was, in fact, nothing that could justly be considered as surprising in this event, yet I acknowledge fairly that it afforded me a degree of childish pleasure, which, were I ambitious of the reputation of a grave philosopher, I ought most certainly rather to hide than to discover.

The quantity of Heat excited and accumulated in this Experiment was very considerable; for, not only the water in the box, but also the box itself, (which weighed $15\frac{1}{2}$ lb.) and the hollow metallic cylinder, and that part of the iron bar which, being situated within the cavity of the box, was immersed in the water, were heated 150 degrees of FAHRENHEIT'S scale; viz. from 60° (which was the temperature of the water, and of the machinery, at the beginning of the Experiment) to 210° , the Heat of boiling water at Munich.

The total quantity of Heat generated may be estimated with some considerable degree of precision, as follows:

Of

Of the Heat excited there appears to have been actually accumulated,

Quantity of ice-cold water which, with the given quantity of Heat, might have been heated 180 degrees, or made to boil.

In Avoirdupois weight.

In the water contained in the wooden box, 18½ lb. Avoirdupois, heated 150 degrees, namely, from 60° to 210° F.

lb.

15.2

In 113.13 lb. of gun-metal, (the hollow cylinder,) heated 150 degrees; and, as the capacity for Heat of this metal is to that of water as 0.1100 to 1.0000, this quantity of Heat would have heated 12½ lb. of water the same number of degrees

10.37

In 36.75 cubic inches of iron, (being that part of the iron bar to which the borer was fixed which entered the box,) heated 150 degrees; which may be reckoned equal in capacity for Heat to 1.21 lb. of water

1.01

N. B. No estimate is here made of the Heat accumulated in the wooden box, nor of that dispersed during the Experiment.

Total quantity of ice-cold water which, with the Heat actually generated by friction, and accumulated in 2 hours and 30 minutes, might have been heated 180 degrees, or made to boil

26.58

From the knowledge of the *quantity* of Heat actually produced in the foregoing Experiment, and of the *time* in which it was generated, we are enabled to ascertain *the velocity of its production*, and to determine how large a fire must have been, or how much fuel must have been consumed, in order that, in burning equably, it should have produced by combustion the same quantity of Heat in the same time.

In one of Dr. CRAWFORD's Experiments, (see his Treatise on Heat, p. 321,) 37 lb. 7 oz. Troy, = 181920 grains, of water, were heated $2\frac{1}{8}$ degrees of FAHRENHEIT's thermometer, with the Heat generated in the combustion of 26 grains of wax. This gives 382032 grains of water heated 1 degree with 26 grains of wax; or $14693\frac{1}{8}$ grains of water heated 1 degree, or $\frac{14693}{180} = 81.631$ grains heated 180 degrees, with the Heat generated in the combustion of 1 grain of wax.

The quantity of ice-cold water which might have been heated 180 degrees, with the Heat generated by friction in the before-mentioned Experiment, was found to be 26.58 lb. Avoirdupois, = 188060 grains; and, as 81.631 grains of ice-cold water require the Heat generated in the combustion of 1 grain of wax, to heat it 180 degrees, the former quantity of ice-cold water, namely 188060 grains, would require the combustion of no less than 2303.8 grains (= $4\frac{1}{8}$ oz. Troy) of wax, to heat it 180 degrees.

As

As the Experiment (N^o 3) in which the given quantity of Heat was generated by friction, lasted 2 hours and 30 minutes, = 150 minutes, it is necessary, for the purpose of ascertaining how many wax-candles of any given size must burn together, in order that in the combustion of them the given quantity of Heat may be generated in the given time, and consequently *with the same celerity* as that with which the Heat was generated by friction in the Experiment, that the size of the candles should be determined, and the quantity of wax consumed in a given time by each candle, in burning equably, should be known.

Now I found by an Experiment, made on purpose to finish these computations, that when a good wax-candle, of a moderate size, $\frac{1}{4}$ of an inch in diameter, burns with a clear flame, just 49 grains of wax are consumed in 30 minutes. Hence it appears, that 245 grains of wax would be consumed by such a candle in 150 minutes; and that, to burn the quantity of wax (= 2303.8 grains) necessary to produce the quantity of Heat actually obtained by friction in the Experiment in question, and in the given time, (150 minutes,) *nine candles*, burning at once, would not be sufficient; for 9 multiplied into 245 (the number of grains consumed by each candle in 150 minutes) amounts to no more than 2205 grains; whereas the quantity of wax necessary to be burnt, in order to produce the given quantity of Heat, was found to be 2303.8 grains.

From the result of these computations it appears, that the quantity of Heat produced equably, or in a continual stream, (if I may use that expression,) by the friction of the blunt steel borer against the bottom of the hollow metallic cylinder, in the Experiment under consideration, was *greater* than that produced equably in the combustion of *nine wax-candles*, each $\frac{1}{4}$ of an inch in diameter, all burning together, or at the same time, with clear bright flames.

As the machinery used in this Experiment could easily be carried round by the force of one horse, (though, to render the work lighter, two horses were actually employed in doing it,) these computations show further how large a quantity of Heat might be produced, by proper mechanical contrivance, merely by the strength of a horse, without either fire, light, combustion, or chemical decomposition; and, in a case of necessity, the Heat thus produced might be used in cooking victuals.

But no circumstances can be imagined, in which this method of procuring Heat would not be disadvantageous; for, more Heat might be obtained by using the fodder necessary for the support of a horse, as fuel.

As soon as the last-mentioned Experiment (N^o 3.) was finished, the water in the wooden box was let off, and the box removed; and the borer being taken out of the cylinder, the scaly metallic powder, which had been produced by the friction of the borer against the bottom of the
the

the cylinder, was collected, and, being carefully weighed, was found to weigh 4145 grains, or about $8\frac{2}{3}$ oz. Troy.

As this quantity was produced in $2\frac{1}{2}$ hours, this gives 824 grains for the quantity produced in *half an hour*.

In the first Experiment, which lasted only *half an hour*, the quantity produced was 837 grains.

In the Experiment N° 1. the quantity of Heat generated, in *half an hour*, was found to be equal to that which would be required to heat 5 lb. Avoirdupois of ice-cold water 180 degrees, or cause it to boil.

According to the result of the Experiment N° 3. the Heat generated in *half an hour* would have caused 5.31 lb. of ice-cold water to boil. But, in this last-mentioned Experiment, the Heat generated being more effectually confined, less of it was lost; which accounts for the difference of the results of the two Experiments.

It remains for me to give an account of one Experiment more, which was made with this apparatus. I found by the Experiment N° 1. how much Heat was generated when the air had free access to the metallic surfaces which were rubbed together. By the Experiment N° 2. I found that the quantity of Heat generated was not sensibly diminished when the free access of the air was prevented; and by the result of N° 3. it appeared that the generation of the Heat was not prevented, or retarded, by keeping the apparatus

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immersed in water. But as, in this last-mentioned Experiment, the water, though it surrounded the hollow metallic cylinder on every side, externally, was not suffered to enter the cavity of its bore, (being prevented by the piston,) and consequently did not come into contact with the metallic surfaces where the Heat was generated; to see what effects would be produced by giving the water free access to these surfaces, I now made the

Experiment, N° 4.

The piston which closed the end of the bore of the cylinder being removed, the blunt borer and the cylinder were once more put together; and the box being fixed in its place, and filled with water, the machinery was again put in motion.

There was nothing in the result of this Experiment that renders it necessary for me to be very particular in my account of it. Heat was generated, as in the former Experiments, and, to all appearance, quite as rapidly; and I have no doubt but the water in the box would have been brought to boil, had the Experiment been continued as long as the last. The only circumstance that surprised me was, to find how little difference was occasioned in the noise made by the borer in rubbing against the bottom of the bore of the cylinder, by filling the bore with water. This noise, which was very grating to the ear, and sometimes almost insupportable, was, as nearly as I could judge of it, quite as loud, and as disagreeable, when the sur-

faces rubbed together were wet with water, as when they were in contact with air.

By meditating on the results of all these Experiments, we are naturally brought to that great question which has so often been the subject of speculation among philosophers; namely,

What is Heat?—Is there any such thing as an *igneous fluid*?—Is there any thing that can with propriety be called *caloric*?

We have seen that a very considerable quantity of Heat may be excited in the Friction of two metallic surfaces, and given off in a constant stream or flux, *in all directions*, without interruption or intermission, and without any signs of diminution or exhaustion.

From whence came the Heat which was continually given off in this manner, in the foregoing Experiments? Was it furnished by the small particles of metal, detached from the larger solid masses, on their being rubbed together? This, as we have already seen, could not possibly have been the case.

Was it furnished by the air? This could not have been the case; for, in three of the Experiments, the machinery being kept immersed in water, the access of the air of the atmosphere was completely prevented.

Was it furnished by the water which surrounded the machinery? That this could not have been the case is evident: *first*, because this water was continually receiving Heat from the machinery, and could

could not, at the same time, be *giving to*, and *receiving Heat from*, the same body; and *secondly*, because there was no chemical decomposition of any part of this water. Had any such decomposition taken place, (which indeed could not reasonably have been expected,) one of its component elastic fluids (most probably inflammable air) must, at the same time, have been set at liberty, and, in making its escape into the atmosphere, would have been detected; but though I frequently examined the water to see if any air bubbles rose up through it, and had even made preparations for catching them, in order to examine them, if any should appear, I could perceive none; nor was there any sign of decomposition of any kind whatever, or other chemical process, going on in the water.

Is it possible that the Heat could have been supplied by means of the iron bar to the end of which the blunt steel borer was fixed? or by the small neck of gun-metal by which the hollow cylinder was united to the cannon? These suppositions appear more improbable even than either of those before mentioned; for Heat was continually going off, or *out of the machinery*, by both these passages, during the whole time the Experiment lasted.

And, in reasoning on this subject, we must not forget to consider that most remarkable circumstance, that the source of the Heat generated by friction, in these Experiments, appeared evidently to be *inexhaustible*.

It

It is hardly necessary to add, that any thing which any *insulated* body, or system of bodies, can continue to furnish *without limitation*, cannot possibly be a *material substance*: and it appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of any thing, capable of being excited and communicated, in the manner the Heat was excited and communicated in these Experiments, except it be MOTION.

I am very far from pretending to know how, or by what means, or mechanical contrivance, that particular kind of motion in bodies, which has been supposed to constitute Heat, is excited, continued, and propagated, and I shall not presume to trouble the Society with mere conjectures; particularly on a subject which, during so many thousand years, the most enlightened philosophers have endeavoured, but in vain, to comprehend.

But, although the mechanism of Heat should, in fact, be one of those mysteries of nature which are beyond the reach of human intelligence, this ought by no means to discourage us, or even lessen our ardour, in our attempts to investigate the laws of its operations. How far can we advance in any of the paths which science has opened to us, before we find ourselves enveloped in those thick mists which, on every side, bound the horizon of the human intellect? But how ample, and how interesting, is the field that is given us to explore!

Nobody,

Nobody, surely, in his sober senses, has ever pretended to understand the mechanism of gravitation; and yet what sublime discoveries was our immortal NEWTON enabled to make, merely by the investigation of the laws of its action!

The effects produced in the world by the agency of Heat are probably *just as extensive*, and quite as important, as those which are owing to the tendency of the particles of matter towards each other; and there is no doubt but its operations are, in all cases, determined by laws equally immutable.

Before I finish this Essay, I would beg leave to observe, that although, in treating the subject I have endeavoured to investigate, I have made no mention of the names of those who have gone over the same ground before me, nor of the success of their labours; this omission has not been owing to any want of respect for my predecessors, but was merely to avoid prolixity, and to be more at liberty to pursue, without interruption, the natural train of my own ideas.

DESCRIPTION *of the* FIGURES.

Fig. 1 shows the cannon used in the foregoing Experiments, in the state it was in when it came from the foundry.

Fig. 2 shows the machinery used in the Experiments N° 1 and N° 2. The cannon is seen fixed in the machine used for boring cannon. W is a strong iron bar, (which, to save room in the drawing, is represented as broken off,) which bar, being united with machinery (not expressed in the figure) that is carried round by horses, causes the cannon to turn round its axis.

m is a strong iron bar, to the end of which the blunt borer is fixed; which, by being forced against the bottom of the bore of the short hollow cylinder that remains connected by a small cylindrical neck to the end of the cannon, is used in generating Heat by friction.

Fig. 3 shows, on an enlarged scale, the same hollow cylinder that is represented on a smaller scale in the foregoing Figure. It is here seen connected with the wooden box (g, h, i, k,) used in the Experiments N° 3 and N° 4. when this hollow cylinder was immersed in water.

p, which is marked by dotted lines, is the piston which closed the end of the bore of the cylinder.

n is the blunt borer seen sidewise.

d, e, is the small hole by which the thermometer was introduced, that was used for ascertaining

ing the Heat of the cylinder. To save room in the drawing, the cannon is represented broken off near its muzzle; and the iron bar, to which the blunt borer is fixed, is represented broken off at *m*.

Fig. 4, is a perspective view of the wooden box, a section of which is seen in the foregoing Figure. (See *g*, *b*, *i*, *k*, Fig. 3.)

Fig. 5 and 6, represent the blunt borer *n*, joined to the iron bar *m*, to which it was fastened.

Fig. 7 and 8, represent the same borer, with its iron bar, together with the piston which, in the Experiments N° 2 and N° 3, was used to close the mouth of the hollow cylinder.

END OF THE SECOND VOLUME.

Fig. 1.



Fig. 2.

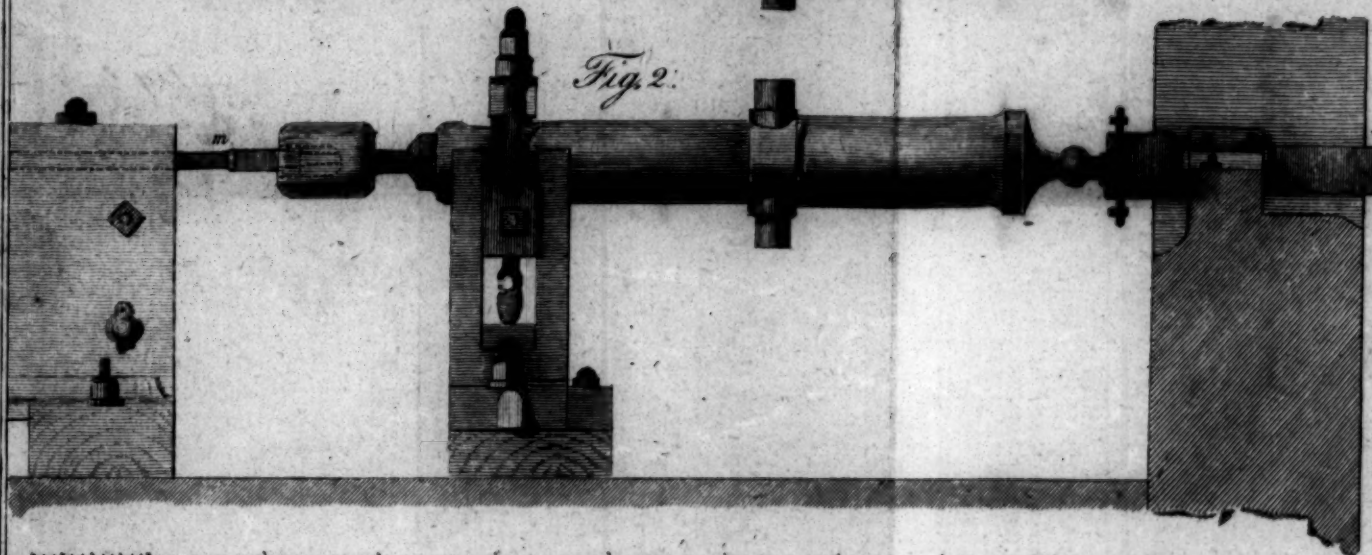


Fig. 3.

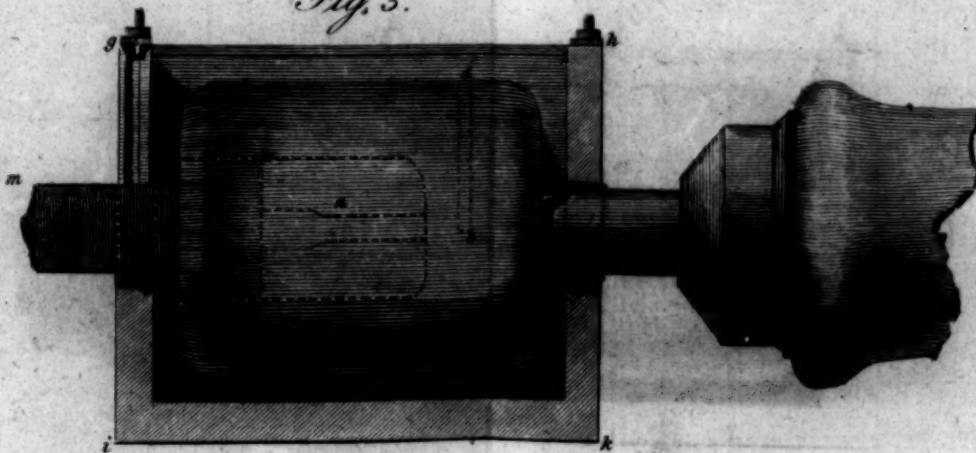


Fig. 5.



Fig. 7.



Fig. 6.



Fig. 8.



Fig. 4.

